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HENRI POINCARE AS AN INVESTIGATOR[1]

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IT has not seemed to me appropriate, nor would there be time, nor should I be able, to enter into an exhaustive study of the life-work of a master-mind like Jules Henri Poincaré. Indeed, to analyze his contributions to astronomy needs a Darwin; to report on his investigations in mathematical physics needs a Planck; to expound his philosophy of science needs a Royce; to exhibit his mathematical creations in all their fullness needs Poincaré. Let it suffice that he was the pride of France, not only of the aristocracy of scholars, but of the nation. He was inspired by the genius of France, with its keen discernment, its eternal search for exact truth, its haunting love of beauty. The mathematical world has lost its incomparable leader, and its admiration for the magnitude of his achievements will be tempered only by the vain desire to know what visions he had not yet given expression to. Investigators of brilliant power for years to come will fill out the outlines of what he had time only to sketch. His vision penetrated the universe from the electron to the galaxy, from instants of time to the sweep of space, from the fundamentals of thought to its most delicate propositions.

In his funeral oration, Painlevé, speaking for the *Académie des Sciences,* said:[2]

> He was only twenty-four years of age, when after four years of silent and sustained reflection, he began the series of mathematical publications which leaves us in doubt whether to admire most its surprising profundity or its surprising fecundity.
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> Whether he attacked the ascension, step by step, of the truths of arithmetic discontinuity, or unloosed the tangle of geometric form, or followed the subtlest

[1] For a biographical sketch of Poincaré, see *Revue des deux Mondes,* 1912, September 15. Also the second edition of Lebon's book on Poincaré has appeared.

[2] *Revue du Mois,* Vol. 7 (1912), p. 133.

windings and caprices of the continuous laws that join quantities together, there is not one of his works which has not the masterly touch, not one of his fifteen hundred publications which does not show the lion's claw.

At the age of twenty-seven, the Faculty of Sciences offered this young conqueror its chair of physical mechanics. At thirty-three the Academy of Sciences opened its door, an example soon followed by the learned academies of the entire world; for there was no body of scientists in Europe or America which did not feel that it honored itself in adjoining the cooperation of Henri Poincaré.

But the mathematical sciences were for this illustrious analyst only a manifold and prodigious measuring instrument admirably adapted to the comparative study of the phenomena of the universe. This instrument he set himself to use, and what skill he displayed! At the age of thirty, he astonished the physicists by his critique of the general principles of their science; that was but the beginning of bold speculations which led him year by year up to the very edge of the unknown, to the constitution of matter, to the paradoxical mechanics that sprung up after the unexpected discovery of the mysterious radioactivity.

Yet this was only part of his activity: geodesy, cosmogony, astronomy, philosophy of science, he included them all, penetrated all, explored all. His celestial mechanics would be glory enough. It was this that revealed him first to a wide public. King Oscar II. of Sweden, Mæcenus of science, enlightened and generous, in 1887 opened an international competition in mathematics. In 1889, at the end of the contest, France learned with joy that the great gold medal, supreme prize of this new tournament, had been awarded to one of her children, a young scientist thirty-five years of age, for a marvelous study of the mechanical stability of our universe; and the name of Henri Poincaré was famous.

Gentlemen, the Theban hero dying after two victories said: "I leave two immortal daughters." This hero of the world of thought who has just succumbed, leaves in the ideal world, as real as the other world, an immortal posterity which will guide the future researches of men. Indeed his life will remain an example as harmonious in its faultless lines as the orbits of those stars whose eternal past and eternal future he desired to know.

To this eulogy of Professor Painlevé certainly I could add nothing, and it does not seem necessary to enumerate the many other honors of Poincaré's. I shall undertake only to consider briefly his conception of science in its chief phases, and in the light of this conception to consider at more length in particular his ideas of research. As an investigator his opinions carry extraordinary weight, as he was a subtle philosopher and a skilled psychologist. We may treat three phases of scientific activity as distinct, pure science, industrial science and what we may call euthenic science.

In speaking of the death of Brouardel,[3] who did much for the study of hygiene, and had helped in preventing three invasions of cholera, without disturbing commerce, Poincaré said before the *Académie des Sciences:*

In this direction scientists can scarcely count on the satisfaction of discovering general laws, exterior as it were to space and time, but there are other

[3] *C. R.*, 143 (1906), p. 996.

joys and above all that of doing good immediately to humanity and correcting evils without forcing the remedy to wait.

The scientist is accustomed to conquer truth only by degrees; for him all certainty should be bought by long hesitations, by perpetually feeling his way. He suspects what comes too easily, and accepts it only after submitting it to numerous and diverse proofs. The man who must act can not embarrass himself by such scruples. He cares little for a truth which must wait so long, because it may arrive too late, and after the moment for action has passed. He must make rapid conquests; sometimes these are not the most durable nor those we should esteem. He also has to avoid reefs which we know not, we for whom time does not count, and sometimes we are tempted to say a true scientist ought not to risk them; how much better on the contrary to congratulate ourselves that there are men skilful enough to avoid them.

Towards pure science his attitude was almost adoration. It is best set forth by extracts from his "Value of Science" and "Science and Method":

The search for truth should be the goal of our activity; it is the only end worthy of it. . . . When I speak here of truth doubtless I mean primarily scientific truth, but I wish to speak also of moral truth, one of whose aspects is what we call Justice. . . . To find one as well as to find the other, it is necessary to struggle to the utmost to free ourselves from the bonds of prejudice and passion, to attain absolute sincerity.

The best expression of the harmony of nature is Law. Law is one of the most recent conquests of the human mind. Man demands that his gods prove their existence by miracles, but the eternal marvel is that there are not miracles all the time. And the world is divine because it is harmonious. Were it ruled by caprices what could ever prove it due to aught but chance?

But does this harmony which the human intellect believes it finds in nature exist outside the intellect? Doubtless not; a reality completely independent of the mind that conceives it, sees it, feels it, is an impossibility. What we call objective reality is, in the last analysis, what is common to many thinkers and could be common to all; this common part, we shall see, can be only the harmony expressed by mathematical laws.

So we conclude that this harmony is the sole objective reality, the sole truth we can ever attain, and if I add that the universal harmony of the world is the source of all beauty, it becomes comprehensible how we should prize the slow and painful progress by which we learn little by little to know it.

The scientist does not study nature because it is useful; he studies it because it pleases him, and it pleases him because it is beautiful. Were nature not beautiful, it would not be worth knowing, life would not be worth living. I do not mean here, of course, that beauty which impresses the senses, the beauty of qualities and appearances; not that I despise it—far from it; but that has nought to do with science; I mean that subtler beauty of the harmonious order of the parts which pure intellect appreciates. This it is which gives a body, a skeleton as it were, to the fleeting appearances that charm the senses, and without this support the beauty of these fugitive dreams would be but imperfect, because it would be unstable and evanescent. On the contrary intellectual beauty is self-sufficient and for its sake, rather than for the good of humanity, does the scientist condemn himself to long and tedious labors.

In connection with this view of the scientist in his own domain, I desire to quote also from the preface of the recent second German edition of "Value of Science," which expresses his attitude towards industrial science:

> Science has always had to contend with skeptics and scoffers who were quite ready to draw conclusions from relative failures and temporary inactivity, and to note the confessions of scientists who admit that the field of science is bounded, but fail to add that inside its own realm it is supreme.
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> He who views scientific work from the outside is often amazed to see yesterday's truth so easily become to-morrow's error. He believes then, that our conquests are over-confident, that the principles so proudly paraded are only novelties, and he does not see that beneath these necessary changes of form scientific truth is always one and the same. It remains eternally unchanged and only the clothing in which we deck it out changes with the fashion.
>
> Fortunately science is needed in applications, and this silences the skeptic. If he desires to use some new discovery, and convinces himself of its success, he must indeed admit that it is more than an idle dream. We thus perceive the blessing which lies in the development of industry.
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> I do not wish to say that science is created for its applications, far from it; one must love it for its own sake; but the recognition of its applications protects us from the skeptic.

Poincaré's conception of science can be summed up in these terms: Science consists of the invariants of human thought.

In the field of investigation, the important thing for Poincaré was the discovery of the real relation between isolated facts. The important facts are those that suggest relations. We select facts from this standpoint. The world of relations was as real to him as the world of phenomena, and so far as we know the real relations, in whatever language we express these relations, just so far we know the actual world, the objective world. Even absolute space and absolute time do not exist, these two are relations furnished by our own minds.[4] Thus the term energy, and our notion as to the existence of energy, may change in the course of time, but the persistent relation that gives us our present notion of energy is real and does not change. It may be true, as Herschel said, that in the twinkling of an eye a molecule solves a differential equation which if written out in full would belt the globe, but the molecule knows nothing of the equation—that is created by the mind, and as the modern discontinuous physics develops, it may be that we shall have to use difference equations rather than differential equations. But the differential equation expresses certain persistent relations between phenomena, and is thus real, and is the replica of an objective reality. The differential equation means that the phenomenon is one such that each state is the result of the immediately preceding state; the new integro-differential equation of Volterra means that the state is due to all the preceding states; the difference equation means that the states follow each other

[4] *Scientia*, 12 (1912), 159–171 (posthumous).

abruptly; and integro-difference equations would mean that they depend on all preceding states discontinuously. Each is able to account for certain relations in the states. In the same sense the word atom is the name for a set of relations, and though it may change and the atom itself become a solar system, yet what we really mean by the atom is permanent and represents an objective reality. We are witnesses too of an evolution in science and mathematics from the continuous to the discontinuous. In mathematics it has produced the function defined over a range rather than a line—a chaos, as it were, of elements—and the calculable numbers of Borel. In physics it has produced the electron, the magneton, and the theory of quanta,[5] about which Poincaré said shortly before his death:

> A physical system is capable of only a finite number of distinct states; it abruptly jumps from one state to another without passing through the intermediate states.

In biology we have the corresponding theory of mutations. Yet despite this apparent reduction of old ideas into dust, contradictory to our hopes of its permanence; as Poincaré put it: this is right and the other is not wrong. They are in harmony, only the language varies; both set forth certain true relations.

Just as Maxwell and Kelvin were able to invent mechanical models of the ether, so Poincaré is perhaps the most profound genius the world has ever known in devising the more subtle machinery of thought to represent the relations he found not only between numbers and geometric figures, but between the phenomena of physics. His mind seemed to create new structures of this kind continually, finding expression for the most intricate relations. Nowadays this is the same as saying that he was a mathematician, for this ideal world of relations is the one with whose structure mathematics is concerned, and which mathematics claims sovereignty over, verifying Gauss's assertion: "Mathematics is Queen of all the Sciences."

In the address of Masson when Poincaré was made one of the forty immortals, he said:

> You were born, you have lived, you will live, and you will die a mathematician; the vital function of your brain is to invent and to resolve more cases in mathematics; everything about you relates to that. Even when you seem to desert mathematics for metaphysics, the former furnishes the examples, the reasoning, the paradoxes. It is in you, possesses you, harries you, dominates you; in repose, your brain automatically pursues its work, without your being aware of it—the fruit forms, grows, ripens, and falls, and you have yourself told us of your wonder at finding it in your hand so perfect. You furnish an admirable example of the mathematical type. Since Archimedes it is classical but legendary. Rarely will historian have found an occasion so fit to note in life its external characters, and in place of relating your works, rather is not this

[5] See *Jour. de Physique*, 1912 (5), 2; pp. 5–34; 347–360.

the occasion to see how mathematical genius manifests itself, whether it is the result of atavism, or the product of a special culture, at what moment and under what conditions it sees light, at what epoch of life it is most active and brilliant?

Fortunately the answer to Masson's question is to be found in Poincaré's own writings, and it becomes the more interesting when taken in connection with his further thesis that the method of research in mathematics is precisely that of all pure science. This method I desire to consider at some length, for I conceive that such a consideration will be entirely appropriate in this place.

The first research mentioned by Rados in the report of the committee to the Hungarian Academy in 1905, when Poincaré was awarded the first Bolyai prize as the most eminent mathematician in the world, is the series of investigations relating to automorphic functions. These functions enable us to integrate linear differential equations with rational algebraic coefficients, just as elliptic functions and abelian functions enable us to integrate certain algebraic differentials. With regard to these researches, Poincaré tells us that for a fortnight he had tried without success to demonstrate their non-existence. He investigated a large number of formulæ with no results. One evening, however, he was restless and got to sleep with difficulty; ideas surged out in crowds and seemed to crash violently together in the endeavor to form stable combinations. The next morning he was in possession of the particular set of automorphic functions derived from the hypergeometric series; he had only to verify the calculations. Having thus found that functions did exist of this kind, he conceived the idea of representing these functions as the quotients of two series, analogous to the theta series in elliptic functions. This he did purely by the analogy, and arrived at theta-fuchsian functions. Having occasion to take a journey, mathematics was laid aside for a time, but in stepping into an omnibus at Coutances, the idea flashed over him that the transformations which he had used to define these automorphic functions were identical with certain others he had used in some researches in non-euclidean geometry. Returning home he took up some questions in the theory of arithmetic forms, and with no suspicion that they were related to the fuchsian functions or the geometric transformation, he worked for some time with no success. But one day while taking a walk, the idea suddenly came to him that the arithmetic transformations he was using were essentially the same as those of his study in non-euclidean geometry. From this fact he saw at once by the connections with the arithmetic forms that the fuchsian functions he had discovered were only particular cases of a more general class of functions. He laid siege now systematically to the whole problem of the linear differential equations and the fuchsian functions and reached result after result, save one thing which seemed to be the key-

stone of the whole problem was stubborn. He was compelled to go away again to perform military duty, and his mind was full of other things. But one day while crossing the boulevard the solution of the last difficulty suddenly appeared and upon verification was found to be correct.

In this account of the birth and growth of mathematical development, which he assures us is practically the same as for all such developments, it is obvious that the central notion is that of generalization. Elliptic, abelian and theta functions are in turn generalized into a new class of transcendents. Inversion of differentials is generalized into inversion of differential equations. This notion of generalization we need to inspect a little closely. Mathematical generalization consists of two types of thought, often not discriminated, and often scarcely to be discriminated from each other. One type consists in so stating a known theorem that it will be true of a wider class than in its first statement, and the predicate asserted has a wider significance. In such generalization the first statement of the theorem becomes a mere particular case of the second statement. Examples will occur readily to every one. There are two forms of this type: in one, many known cases are brought together under one law; in the other form, the law thus found is made to apply to other known cases, perhaps never before suspected to be related to the first set. It is the guiding threads of analogy that usually bring about these forms of generalization. This kind of generalizing power Poincaré had in high degree. In his memoir on "Partial Differential Equations of Physics"[6] he says:

> If one looks at the different problems of the integral calculus which arise naturally when he wishes to go deep into the different parts of physics, it is impossible not to be struck by the analogies existing. Whether it be electrostatics, or electrodynamics, the propagation of heat, optics, elasticity or hydrodynamics, we are led always to differential equations of the same family; and the boundary conditions though different, are not without some resemblances. . . . One should therefore expect to find in these problems a large number of common properties.

Also in his "Nouvelles Méthodes de la Mécanique Céleste" he says:

> The ultimate aim of celestial mechanics is to solve the great question whether Newton's law alone will explain all astronomical phenomena.

In his address awarding Poincaré the gold medal of the Royal Astronomical Society, G. H. Darwin[7] said:

> The leading characteristic of M. Poincaré's work appears to be the immense wideness of the generalizations, so that the abundance of possible illustrations is sometimes almost bewildering. This power of grasping abstract principles is the mark of the intellect of the true mathematician; but to one accustomed rather to deal with the concrete the difficulty of completely mastering the argument is sometimes great.

[6] *Amer. Jour. Math.*, Vol. 12.

[7] "Scientific Papers," Vol. 4, p. 519.

In the account of the creation of the fuchsian functions we see this power of finding examples of his generalizations, that is to say, of applying them. By these functions he could solve differential equations, he could express the coordinates of algebraic curves as fuchsian functions of a parameter, he could solve algebraic equations of any order. Humbert put it succinctly thus: "Poincaré handed us the keys of the world of algebra." Again, from the simplification of the theory of algebraic curves he was able to reach results which led to a generalization of the fuchsian functions to the zetafuchsian functions, which he afterward used in differential equations, the starting point of the problem. He applied the theory of continuous groups to hypercomplex numbers and then applied hypercomplex numbers to the periods of abelian integrals and the algebraic integration of differential equations of certain types. He applied fuchsian functions to the theory of arithmetic forms and opened a wide development of the theory of numbers. He applied fundamental functions to the potential theory of surfaces in general, showing how the Green's function could be constructed for any surface, permitting the solution of the problem. He developed integral invariants, which persist through cycles of space and time. He dared to apply the kinetic theory of gases and the theory of radiant matter to the Milky Way itself, suggesting that probably we are a speck in a spiral nebula. He analyzed mathematically the rings of Saturn into a swarm of satellites, and the spectroscope confirmed his conclusions, a piece of work ranking with the mathematical discovery of Neptune. He found a generalization for figures of equilibrium of the heavenly bodies, discovering an infinity of forms and pointing out the stable transition shapes from one type to another, of which the piriform was quite new; at the same time throwing light on the problems of cosmogony. He applied trigonometric series, divergent series, and even the theory of probabilities, to show that the stability or instability of our universe has never been demonstrated, but that if probability is measured by continuous functions only, the universe is most probably stable.

There is no essential difference between generalizations of this type in whatever realm they appear. It is generalization to see that projective geometry merely states the invariancies of the projective group, and elementary geometry is a collection of statements about the invariants of the orthogonal group. Expansions in sines and cosines, or Legendrian polynomials, or Bessel functions are particular cases of expansions in fundamental functions, and these arise from the inversion of definite integrals. It is also generalization to reduce the phenomena of light to a wave-theory, then the phenomena of light, electricity and magnetism to ether-properties. It is generalization to reduce physics and physical chemistry to the study of quanta of energy, and, I might add, to reduce all the physical sciences to a study of the kinematics of four-dimensional

space. When we say natural law, we mean generalization of this type. The laws of science are generalizations of the relations between phenomena. According to Poincaré there are three classes of hypotheses in science: (1) Natural hypotheses, which are the foundations of the mathematical treatments, such as action decreases with the distance, small movements follow a linear law, effect is a continuous function of the cause, physical phenomena are discontinuous functions; (2) Neutral hypotheses, which enable us to formulate our ideas, and are neither verifiable nor unverifiable, such as the hypothesis of atoms or of a continuous medium; (3) Generalizations, invariantive relationships, which are valuable, may be verified by experiment and lead to real progress. In "Science and Hypothesis" his thesis is, that science consists of observed facts organized according to these three classes of hypotheses. In "Value of Science" the thesis is, that the objective value of science consists in the laws, that is, in the generalizations, discovered. In "Science and Method" the thesis is, that the discovery of laws is by methods substantially the same as those of mathematical investigation, deducing from a significant particular a wide-reaching generalization, selecting our facts because of their significance.

This type of generalization, however, is only a part of the mathematical generalization. It might in broad terms be characterized as the purely scientific type. The second type, which might be broadly characterized as the purely mathematical type, is that in which there is a distinct widening of the field of a conception, usually by the addition of new mathematical entities. Examples are the irrational numbers, negative numbers, imaginary numbers, quaternions and hypercomplex numbers in general. The name imaginary indicates the fact that the actual existence of these was once open to question in the minds of some. Other examples are the non-euclidean geometries, the non-archimedean continuity, transfinite numbers, space of four and of N dimensions. The ideal numbers of Kummer and the geometric numbers of Minkowski are generalizations mainly of this type. It is not possible to separate sharply this kind of generalization from the other, and it would often be difficult to say whether a given mathematical investigation belongs primarily to the one kind or the other. However, when an investigation does not merely utilize material that is already known, but introduces new objects for study whose properties are not known, we can classify it under the second type. Usually the second type arises from inversion processes. We have given certain properties to find the class of things satisfying them. If they do not exist we create them. Whether we consider that the new objects have (in mathematics) been created or discovered, is merely a matter of psychologic point of view. For example, in one of Poincaré's last papers[8] he explains the apparently irreconcil-

[8] *Scientia*, 12 (1912), pp. 1–11.

able difference of opinion which there is among mathematicians regarding the existence of a definable infinity as due to the difference in the psychology of the two classes. One, the idealistic, feeling that everything we define is due to the mind, and finite; the other, the realistic, feeling that there is an external world which may well contain an infinity. The idealistic class, to which Poincaré belonged, would consider that these extensions to which we referred are in a sense creations.

It is scarcely necessary to enumerate the creations of Poincaré. They are many, for he was gifted with extraordinary originality. The account given above of the creation of the fuchsian functions is an example of one of his most important. It opened an immense field of investigation. He created a type of arithmetic invariants expressible as series or definite integrals, which opened a new field in theory of numbers. His investigations of ordinary differential equations which are not linear, such as those in dynamics and the problem of N bodies, created an extensive class of new functions which (I believe) are yet without special names, as well as suggesting the existence of classes of functions for which we have, as yet, even no means of expression. The investigations of asymptotic expansions opened paths to dizzy heights. Fundamental functions in partial differential equations also open a region now under development. We may say that the most marvelous of his creations rise from the general field of differential equations. We might cite further his researches in analysis situs, the realm of the invariants of a battered continuity. His double residues and studies in functions of many real variables are creations from which will spring a noble progeny. Even the lectures in which he presented the results of others scintillate with original thoughts.

To generalize in mathematics and science it is not enough simply to get together facts or ideas and to put them into new combinations. Most of these combinations would be useless. The real investigator does not form the useless combinations at all, but unconsciously rejects the unprofitable combinations. It is as if he were an examiner for a higher degree; only the candidates who have passed the lower degrees ever appear before him at all. Often domains far distant furnish the useful combinations, as in the account given of the genesis of the fuchsian functions, the theory of arithmetic forms through the roundabout route of non-euclidean geometry furnished the generalization of the first fuchsian functions to the complete class. This was of the first type. But how are those of the second type born?

We come thus to the heart of the matter. Merely to say that we discover laws is not sufficient. How do we discover extensions? How devise new formulæ? Make new constructions? The answer to this question is, for Poincaré, found in psychology. It is necessary to get together many facts, but this does not insure that we shall build with

them any more than that a collection of beams and stones will make a cathedral. Mere haphazard construction does not produce the cathedral either. To reach the end it is necessary to have the end in view from the beginning. It is not only necessary to choose a route, but we must see that it is the route to be chosen. This implies a power of the mind which Poincaré calls intuition. It is that power which enables us to perceive the plan of the whole, to seize the unity in the matter at hand. This power is necessary not only to the investigator, but it is also necessary—in less degree, perhaps—to him who desires to follow the investigation. Why is it, he asks, that any one can ever fail to understand mathematics? Here is a subject constructed step by step with infallible logic, yet many do not comprehend it at all. Not on account of poor memory—that may lead to errors in calculation, but has little to do with comprehension of the subject. Sylvester, for example, was notorious for his inability to remember even what he himself had proved. It is not due to lack of the power of attention, for while concentration is necessary in the development of a demonstration, or in following a piece of logic, it does not give this appreciation of mathematics. A mathematical demonstration is a series of inferences, but it is above all a series of inferences in a certain order. The important thing is the order, just as in chess the mere moving according to the rules is not enough, it is the plan of the game that counts. If one appreciates it, this order, this plan, this unity, this harmony, he need have no fear of a poor memory, nor need he weary his concentration. The student deficient in this power may learn demonstrations by heart, he may assent to each step as logically proved, yet he will know little of the theorem itself. Those who possess this kind of insight which reveals hidden relations, this divining power for the discovery of mines of gold, may hope to become investigators, creators. Those who do not have it must find it or give up the task. The great educational question of the day is the problem of the development of the intuition. If we learn to cultivate this spirituelle flower it will open all doors of invention and discovery of laws. It is an interesting problem for even the grade teacher. If it be true, as Boris Sidis and others have claimed, that there are superior methods of education (which seem really to lie along this line) then they must become the methods of future education. We will begin to educate for genius. One thing seems evident, that too prolonged adherence to the methods of rigid reasoning leads to sterility. In mathematics at least both logic and intuition are indispensable, one furnishes the architect's plan of the structure, the other bolts it and cements it together. Logic, says Poincaré, is the sole instrument of certitude, intuition of creation. Yet even the steps of a logical deduction are planned in their entirety by the intuition. In discussing the partial differential equations of physics[9]

[9] *Amer. Jour. Math.*, Vol. 12.

and their solutions, he points out that one often has to content himself with the guidance he can get by physical considerations. An example of this was the use made by Klein of electrical considerations in handling Dirichlet's problem on a Riemann surface. In the physical aspect of the problem this would usually be sufficient, for the physical data are at the best only approximate. The mathematical necessities of convergence demand, however, that the problems be handled purely analytically and deductively. In one of his lectures he compares the process with the formation of a sponge. When we find it fully formed it is only a delicate lace-work of needles of silica. But the really interesting thing is the form it has taken, and this can be fully understood only by knowing the life-history of the sponge which has impressed its form, its will, so to speak, on the silica. In the same way a logical development of a theorem can really be understood only through a study of its living development. Need we point out the significance of this to the research student? Just as a painter who would become great must sit at the feet of a master and see his creations grow on the canvas, so the student does well to watch a master at work on scientific creations. This is the good he gets at the university. No compendium of results of the great creators will suffice. Nor is a too detailed study of the history of a problem, or too extensive a list of its bibliography, of assistance to the intuition. These might assist the later logical development, but not the inventive power. Poincaré rarely did more than to acquaint himself with the present status of a problem he desired to consider. It is evident too that the intuition is *sui generis,* and guidance of it in the seminar must simply stimulate, not undertake to determine its form. The investigator must set his own problem and work it out in his own way. The director of research should furnish favorable surroundings and set forth the matter of his lectures in as genetic form as possible, as for example, Poincaré's and Klein's masterly courses. But he should not prescribe forms of development, nor methods of attack for the novitiate.

The types of intuition are numerous. We leave to the psychologist their enumeration and description. For example, we should expect a visualist to think in pictures, for in this direction his imagination would be vivid. Such a mind would make use of diagrams and mechanical forms to embody his ideas. We think at once of Faraday and his lines of force, of Kelvin and his models of the ether. Poincaré compares Bertrand and Hermite, schoolmates educated at the same time in the same way. Bertrand when speaking was always in motion, apparently trying to paint his ideas. Hermite seemed to flee the world, his ideas were not of the visible kind. Weierstrass thought in artificial symbols, Riemann in pictures and geometric constructions. Poincaré is spoken of as belonging to the audile type, for he remembered sounds well.

He seems from his memoirs and papers, however, also to be equally of the visual and the symbolic types. He valued words highly, and his style is a mountain brook descending from rarefied heights, its clear current here falling over rocks, there gliding smoothly down. His thought is a penetrating ray that illuminates the deepest recesses of the wilderness of phenomena.

But in any case, whether one be analyst, physicist, biologist or psychologist, the characteristic trait of the intuition is the direct appreciation of relationships between the objects of thought, which unite them into a complete structure, unitary in character and harmonious in form. We might define intuition as that power of the mind by which we build the great theories and fit phenomena into a plan designed along the lines of unifying principles. To be more exact, the mind creates a world of its own. This world is conditioned by what we call the outside world, but in many respects we are free to make it what we please, just as the architect is free to create his building although his material limits him. However, we endeavor to create this world with the maximum simplicity, mainly because simplicity implies harmony, that is, beauty. We are not satisfied with what William James called the "blooming confusion of consciousness" but we construct a replica of this consciousness which is simpler. Of two ways we can construct the replica, we choose the simpler. Thus we choose Euclidean geometry instead of Lobatchevskian, on account of its simplicity, although either might be applied to the world of phenomena. We choose to say the earth rotates on its axis, for that makes astronomy possible. This replica must have a plan, a design, a symmetry, a coherence. Intuition is the perception of this idealized structure. It is akin to the dream of the artist, or the vision of the prophet. Indeed the eminent literary critic, Émile Faguet, calls Poincaré a poet. Was it not Sylvester and Kronecker who said that mathematics was essentially poetry! That was as far as they ever got in defining it. In his address on "Analysis and Physics," Poincaré says:

> Mathematics has a triple end. It must furnish an instrument for the study of nature. But that is not all, it has a philosophic end; and, I dare to say it, an esthetic end; . . . these two ends [physical and esthetic] are inseparable, and the best way to attain the one is to keep the other in view.

The mathematician does not build in stone, nor paint on canvas, nor construct a symphony, though his harmonies are in and through all these; his medium is more ethereal; but is his creation therefore the less beautiful?

Since the intuition is necessary, the first problem of education becomes the conservation and development of this power. Poincaré points out that in mathematics, for example, we should not begin with general definitions and laws, nor with rigorous logic in the proofs of

the theorems. Thus he recommends that in the special mathematics of the secondary school and in the first year of the *Ecole Polytechnique,* there should not be introduced the notion of functions with no derivatives. At most we should content ourselves with saying "there are such, but we are not concerned with them now." When integrals are first spoken of, they should be defined as areas, and the rigorous definition should be given later, after the student has found many integrals. He says:[10]

> The chief end of mathematical instruction is to develop certain powers of the mind, and among these the intuition is not the least precious. By it the mathematical world comes in contact with the real world, and even if pure mathematics could do without it, it would always be necessary to turn to it to bridge the gulf between symbol and reality. The practician will always need it, and for one mathematician there are a hundred practicians. However, for the mathematician himself the power is necessary, for while we demonstrate by logic, we create by intuition; and we have more to do than to criticize others' theorems, we must invent new ones, this art, intuition teaches us.

We turn finally to the research student. How is he to bring the intuition to bear on his problem effectively? If creative work is to be hoped for only through this agency, how do we set it to work? This question Poincaré answers in his analysis of his creation of the fuchsian functions. He holds that the intuition does its work unconsciously. We can not use the term "subconsciously," for he had a repugnance to the doctrine of the superiority of the subliminal self. He points out that our unconscious activity forms large numbers of mental combinations, as an architect, we will say, makes many trial sketches, and of these combinations some are brought into consciousness. These are selected, he believes, by their appeal to the sentiment of beauty, the intellectual esthetic sense of the fitness of things, the unity of ideas, just as the architect from his haphazard sketches selects the right one finally by its appeal to his sense of beauty. Poincaré admits that this explanation of the facts is a hypothesis, but he finds many things to confirm it. One is the fact that the theorems thus suggested in mathematical creation are not always true, yet their elegance, if they were true, has opened the door of consciousness to them. It was Sylvester who used to declare:

> Gentlemen, I am certain my conclusion is correct. I will wager a hundred pounds to one on it; yes, I will wager my life on it.

But it often turned out the next day that it was not true. However, it led eventually to things that were true. The direct conclusion from Poincaré's hypothesis would be that we must conserve and develop the esthetic sense of our field, whether mathematics, physics, chemistry, or what not. And we may well pause to consider whether the young

[10] *L'Enseignement Math.*, 1899, p. 157.

investigator should not include some course in design in his work, in painting, architecture, music, poetry or sculpture. Courses in the appreciation of art, rather than the criticism of art, might also be very serviceable indirectly. The constructive philosophers, like Plato or Bergson, might furnish valuable indirect training. Reading that leads to an appreciation of the beauty and sublimity of the universe is of the same value. In any case whatever would intensify the esthetic sensitiveness would be worth while.

When the intuition does not favor us, the golden butterfly fails to emerge from its chrysalis, what is to be done? Here is his answer for whom time did not count, taken from one of his most recent papers.[11] There is a note of pathos in it as well as a hint of premonition. He presents some incomplete results of a new and very important theorem in geometrical transformation, which he is convinced is true, yet the proof of it encounters great difficulties. Every particular case he has been able to settle is favorable to the theorem. After explaining why he is publishing an incomplete paper for the first time, he says:

> It would seem that in this situation I should abstain from all publication so long as I have not solved the problem, but after fruitless efforts for many months it seems to me wisest to let the whole problem ripen during several years. That would indeed be well, were I sure of some day being able to take it up again, but at my age I can not go bail for this. On the other hand, the importance of the subject is great . . . and the totality of results so far obtained is too considerable for me to resign myself to definitively allowing them to become unfruitful. I may hope that the mathematicians who interest themselves in the problem and who will be more fortunate than I without doubt will find some means to resolve it.

Again, Poincaré points out that these flashes from below the horizon of consciousness must be preceded by periods of prolonged attentive work. It is like setting Pegasus to plowing corn, but this conscious effort is necessary. This discouraging wandering over the hills and rocks, examining the promising paths and the fragments that point to a nearby mine, day after day, is indispensable to success. It is the weary search over the face of the mountain and the driving of many fruitless drifts that eventually lead the prospector to his mine of gold. On this kind of drudgery Poincaré spent two periods of two hours each daily. The unconscious action of his mind did the rest of his work.

Neither does the discovery of the mine develop it. After the unconscious power has led us to our eldorado, it has done all it can. The deductions, the demonstrations, the applications, must be carried out at the expense of prolonged effort again. The intuition can not do this kind of work. Its region is the nebulous part of thought where the mental ions unite, dissolve, and whirl away,—or we may say that

[11] *Rend. Circ. Mat. Palermo*, 33 (1912), p. 375.

it is found where the breakers surge against the shores of the unknown. But in the consciousness, the stable, the crystallized, the permanent combinations are formed; the new world is organized, surveyed, mapped, and the frontier is widened. Here everything proceeds under hard supervision.

Finally, the research student, the investigator, must have a burning love for the search for truth, as well as for the truth itself. And when in his somber moods he asks, what does it signify in the end? he finds the answer at the close of Poincaré's "Value of Science." He expresses the significance of science in these clear terms:

> Civilizations are measured only by their science and their art. Some persons are surprised at the formula: science for science's sake; yet it is quite as good as life for life's sake, if life is only misery; and even as happiness for happiness' sake, if one does not place all pleasures on the same level, if one does not admit that the end of civilization is to furnish more alcohol to people who like to drink.
>
> Every action must have an aim. We have to suffer, we have to work, we have to pay for our seat at the show, but it is in order that we may see, or at least that others may sometimes see.
>
> What is not thought is nought; since we can think only thoughts, and every word we use in talking about things stands for a thought, to assert there is anything else than thought is a senseless affirmation.
>
> Meanwhile—a strange contradiction for those who believe in time—geologic history teaches that life is only an episode between two eternities of death; and even in this episode conscious thought has endured and will endure but a moment. Thought is but a flash in the midst of a long night.
>
> Yet this flash indeed is everything.

A CHRONICLE OF THE TRIBE OF CORN[1]

By Professor EDWARD M. EAST
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ALEXANDRE DUMAS maintained that he weaved more history into his romances than the contemporary chroniclers did into their histories. Perhaps he did. At least the reader may lose himself in the marvelously interesting fancies of the great Frenchman, and if he gleans some points of fact they are gratuitous—features for which he has not paid. But when he finds that his cherished enmity toward Aaron Burr is founded on the fictions of political opponents, that the reformation was largely politics and not ethics, he feels in much the frame of mind as when in earlier days he was robbed of his belief in Saint Nicolas.

These statements are not intended as a libel on the political historian. They serve only to defend the title of this article. The modern historian depends, first upon the records of writers contemporary with the epoch under consideration, second, upon the corroboration or refutation of these records by circumstantial evidence. The biological historian uses precisely the same method. His contemporary records are the records set down by the plants or animals themselves—autobiographies, as it were. He has this advantage over the transcriber of written records, however, the plant autobiographies are true. There is no boasting, no glossing of faults, no exaggeration. The transcriber may misinterpret the record, but this is not the fault of the record. He has but to read it aright. The written record, on the other hand, may be false at the outset.

The story of the birth and evolution of maize, the plant at the basis of our national prosperity, is of interest not only to agriculturists and botanists but to historians and philosophers, for it is one of the crops whose cultivation is linked with the beginnings of civilization. It has taken some years to fit the puzzle together, but now the gaps are but few. Of course the proofs are not absolute. No proof is. But it may be left to the judgment of the reader whether the case is beyond the reasonable doubt of the lawyer. At least, it is typical of the reasoning

[1] An endeavor to trace the exact path of the evolution of maize is beset with more difficulties than are indicated here. I agree with many of the conclusions of both Montgomery and Collins, whose excellent researches have given us a remarkable insight into the probable phylogenetic history of maize. I have endeavored to present in this paper only the probable way in which certain important jumps occurred, facts that might be supposed to be of more popular interest than a strictly botanical discussion.

FIG. 1. A TYPICAL MAIZE PLANT AS DESCRIBED BY THE BOTANISTS. (After Bonafous.)

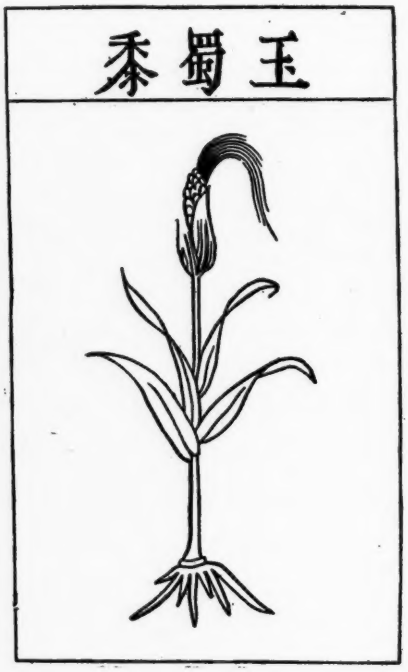

FIG. 2. A REPRODUCTION OF THE CHINESE DRAWING UPON WHICH RESTED THE ARGUMENT FOR ASIATIC ORIGIN OF MAIZE. (After Bonafous.)

of the botanical historian and has been carried further than that of any other plant that has been cultivated since before recorded time and of which the wild prototype is unknown.

The clues upon which the botanical detective works are many, and it is only by dovetailing numerous facts that the probability of a correct conclusion is increased until it is beyond question. That criminal detectives can establish a reasonable proof by circumstantial evidence was shown long ago by Poe. Mathematics does not recognize a series of coincidences. Coincidences do occur but not in series. If a series of facts point to the same conclusion, the probability that that conclusion is correct increases by multiplication, not by addition. If the probability that one throws heads with a coin is one half, the probability that he throws a pair of heads with two coins is one half times one half, or one quarter, not one half plus one half which would be a certainty. Thus the fictitious reasoners of Poe and Doyle have argued that if a series of independent circumstances point in a single direction, that direction is the proper one. If certain facts seem to be outstanding, they must be looked to, for their fallacies will sooner or later come to light. The same is true in botanical history as the following incident shows:

The sagas of Iceland show unquestionably that some time about the year 1000 the Norsemen landed in North America. Where they landed has been a question. The sagas describe the natives they met, the Skrellings, as small and ugly, great of eye and broad of cheek. "And they came in skin canoes." The description fits only the Esquimaux. The sagas relate further, however, that the Norsemen found mösurr wood and self-sown wheat and that in the spring they filled their boats with "wine berries." Students of the sagas have taken the wineberries to be grapes, the self-sown wheat to be wild rice and the mösurr wood to be maple. There were discrepancies here. The ethnologists say the Esquimaux have not wandered south, and the botanists find that the grape and the wild-rice do not grow in the northeast. It may also be pointed out that grapes are not gathered in the spring even in the most flourishing circumstances.

Some have ridiculed the sagas, some have brought the Esquimaux as far south as Boston, others have turned the Skrellings into Indians in spite of their description. It remained for a botanist, Professor M. L. Fernald, to show that the mösurr wood is birch, that the wild wheat is the Strand wheat (*Elymus arenarius*) a plant familiar to the Icelanders, and that the wineberry is either the mountain cranberry that is in its prime in the spring or one of the wild currants, both plants being known to the Norsemen as vínber or wine berries. The plentiful occurrence of these species north of the St. Lawrence River straightens out all the inconsistencies and makes the geography, ethnology and biology of the old sagas perfectly plausible.

This short illustration typifies the method of the botanical historian, though perhaps the details of his work had best be explained. Foremost in the significance of its evidence is the geographical distribution of the wild plant and its subvarieties. From this knowledge one may sometimes locate the point of origin with surprising definiteness. But often an important cultivated species has no known progenitor in the wild. This lack of information is unfortunate for the investigator, but not prohibitive of results. It makes the problem only that much more interesting. The next point of attack is to discover the distribution of the wild species nearest related by their structure and characteristics to the material under investigation. The fact that an organic evolution has occurred is the master key that unlocks many problems. Classification along natural lines was made possible by establishing the fact of evolution. The relatives of plants are hall-marked in a manner not often mistakable, and if the general family group is not too widely distributed, the problem may be considered as fairly well along.

If there are no near relatives extant, if the plant is the last leaf upon the family tree, one must turn to the evidence of the plant itself. By this I mean he must study the inheritance of the various characters

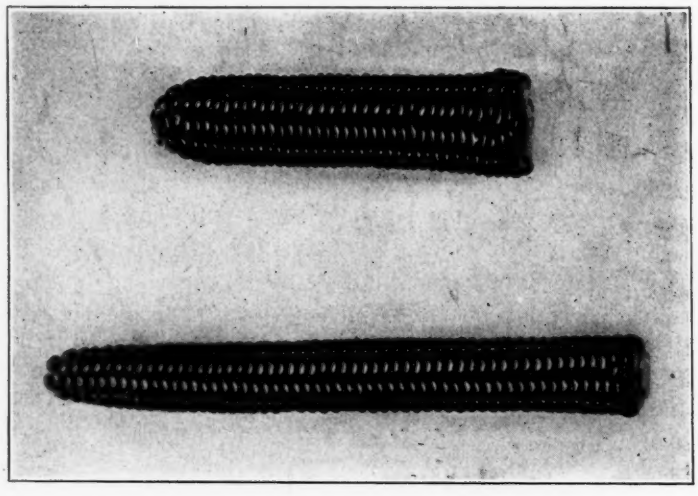

FIG. 3. AN ANCIENT INDIAN FLINT TYPE ABOVE, WITH ITS MODERN SUCCESSOR BELOW.

by which its varieties are differentiated and endeavor to find out how the features peculiar to it have originated. He may then be able logically to connect it with very distant relatives.

Now to turn to the collateral evidence. Collateral evidence on distribution and relationship is furnished by paleontology. Such data are really direct and important when fossil remains occur in sufficient quantities, but this is not often the case. It is usually fragmentary and can be classed with that of archeology. Neither archeology nor history furnishes certain proof of plant origin, however, as we shall see. Their evidence must simply be given the weight it deserves when considered with other facts. Lastly, philology furnishes indications as to the history of a species, for common names of cultivated plants are well preserved in the languages of the people who have used them. But, like other evidence, it must be accepted with caution. The cashew is called by the French *pomme de Mahogani,* which is all right except that it is not an apple and has nothing to do with mahogany. This shows how much worse a compound name is than a simple name, since with a simple name there can be but one error.

We shall endeavor to construct our history and evolution of maize along these lines, though not keeping the same order.

Maize has not been found in the wild state, although it is such a remarkable plant it seems improbable that with our present knowledge of plant distribution it should remain undiscovered if in existence. This fact has made the problem of its nativity very difficult, even though Americans have been satisfied of its new-world origin for some time. Competent critics have skillfully argued old-world origin, and from the strictly historical point of view there was earlier much to be

said in their favor. The word maize (mays) itself is strictly American, but this name has been in use only since adopted by Matthiole in 1570. In modern European languages the common name has been one purporting to show eastern origin, in English Indian corn, in French *blé de Turquie* or Turkish wheat. Since maize is not wheat, it might almost be concluded it was not Turkish. The trouble was, one could not prove it. As a matter of fact, such names only show the tendency of a people simply to indicate the foreign origin of an introduced article, as when the French gave the name coq d'Inde or Indian cock to the American turkey. According to De Candolle maize was called Roman corn in Lorraine and Vosges, Sicilian corn in Tuscany, Indian corn in Sicily and Spanish corn in the Pyrenées. The Turks call it Egyptian corn and the Egyptians, Syrian *dourra,* which prove it to be neither Egyptian nor Syrian.

It has been generally agreed by historians that there was no Hebrew or Sanskrit word for maize and that there was no Egyptian representation of the plant. It is true, Rifaud found an ear of maize in a tomb at Thebes, but this was the work of a modern impostor, for if maize had been a crop of ancient Egypt, pictures of it would have been as plentiful as they are of other Egyptian plants. The plant certainly was not known in Europe in early times, but the question ever arose whether or not it could have been introduced from the East during the Middle Ages. Bonafous, who was the foremost writer on the subject in the early nineteenth century, took this view and was responsible for long-continued doubt on the subject. The principal evidence on the question was that obtained from a charter drawn up between two crusaders in 1204, according to which seeds thought to be maize and brought from Anatolia were presented to the town of Incisa. Historians of the crusades made much of this charter, although botanists thought from

FIG. 4. A GIANT FLOUR CORN FROM PERU COMPARED WITH A DWARF POP CORN FROM THE UNITED STATES.

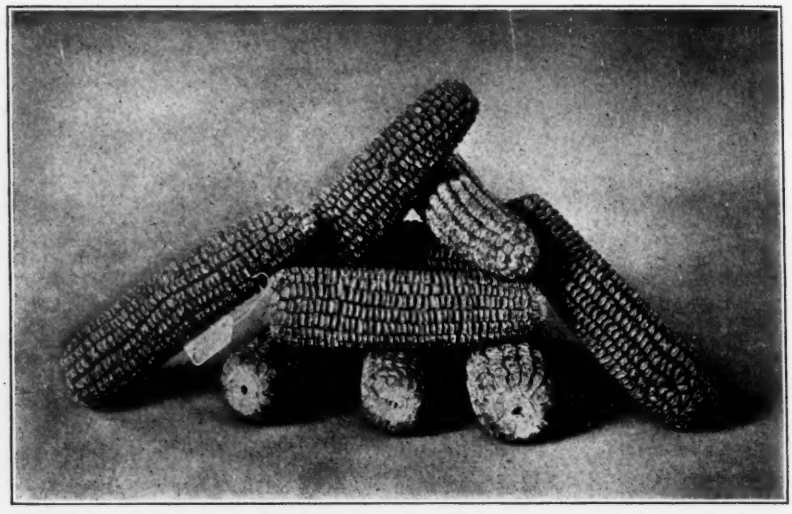

FIG. 5. A PRIZE-WINNING SWEET CORN.

their description that the seeds might be sorghum instead of maize. The absurdity of relying on such isolated clues came out with the discovery that the whole charter of Incisa was a modern fabrication.

The only other evidence of eastern origin that there has been any trouble in demolishing is a picture of an ear of maize together with its ideograph in a Chinese book written some time between 1578 and 1597. Since the Portugese came to China in 1516 and to Java 20 years earlier, it is plain this is not good evidence of Chinese origin. During the half-century between this date and the date of the article, nothing could be more probable than Portugese introduction of maize into China. Furthermore, the fullness of early Chinese records is such that they would hardly have remained silent on an important agricultural crop until 1578.

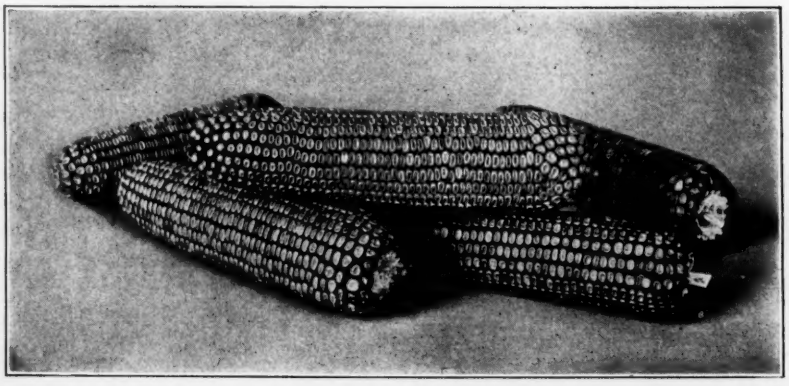

FIG. 6. A "CHAMPION" SAMPLE OF OUR GREATEST ECONOMIC TREASURE. THE IMPROVED DENT CORN.

This dearth of early records of the plant in the old world shows convincingly the American origin of the plant, for after the discovery of America its cultivation became rapidly diffused, a proof that if indigenous to Asia it would have been important agriculturally for centuries.

On the other hand, no one has ever questioned the fact that maize was widely cultivated in America at the time the country was discovered by Europeans. It was the staple crop in both continents and had names in all the native languages. Its antiquity and importance are evidenced by its prominence in the religious rites of the people. The North American burial mounds, the tombs of the Incas and the temples of Mexico were made depositories of the seeds just as the tombs and temples of Egypt treasured wheat and barley. These facts do not indicate antiquity in cultivation equal to that of Egypt, however, for the

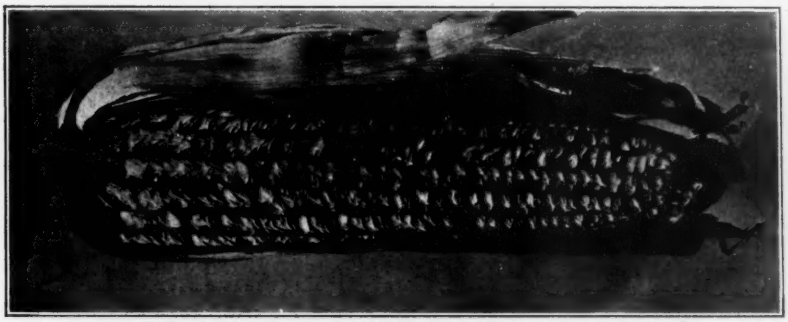

FIG. 7. AN EAR OF THE MEXICAN PODDED CORN (*Zea mays tunicata*).

civilization of the Peruvians and Mexicans is known to be of a much later era. At the same time, one may assume a history much longer than that indicated by these data for two reasons: from its wide distribution and numerous ancient varieties, and from Darwin's discovery of its seeds mixed with shells buried in soil along the Peruvian shore that had become raised by natural action 85 feet above sea level.

The American origin of maize being assured, interest in our problem narrows. The Americas are large. To what particular part was the plant indigenous? First let me say that it is a peculiar fact that the vast territory now known as the United States produced no cultivated plants of first importance. Excluding the Jerusalem artichoke, some comparatively unimportant berries and some relatives of the apple, our country gave man no agricultural treasures. It merely accepted with thanks the lavish generosity of the tropics. As far as maize is concerned, the physiology of the plant itself corroborates this statement. It germinates and grows best in hot climates. We must look for the home of maize, therefore, in the plains or plateaus of tropical North

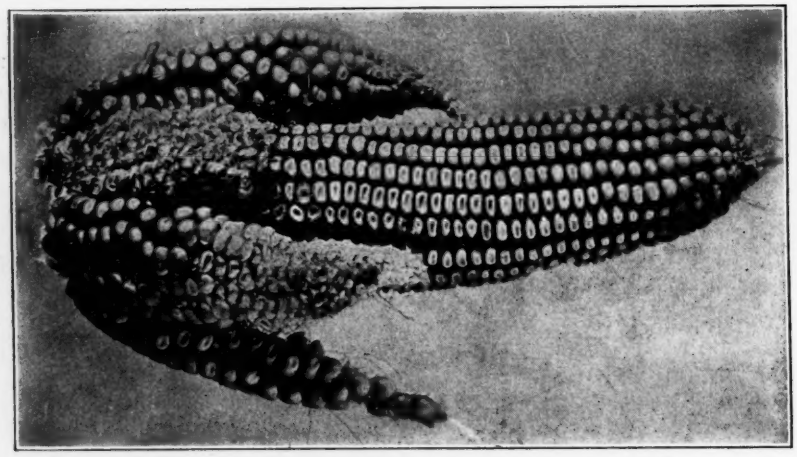

FIG. 8. A REVERSION TO THE ANCIENT BRANCHED EAR TYPE.

or South America—the plains because annuals do not develop in forested regions. Under the circumstances, our search need only be the less troublesome absentia search in botanical records, since the regions have been combed by botanical explorers for three hundred years. The result as far as maize is concerned was *nil*. Perhaps though the word nothing is too exclusive. First cousins of our interesting family were discovered in Mexico and Guatemala, the plant called teosinte; and experimental evidence indicates a sufficiently near relation to justify these regions as the original home of the emigrant. This evidence, which gives us a picture of the original plant, is now to be considered.

Maize varieties differing slightly from each other are now numbered by the hundred. Of these, five or six differ by very distinct characters and have come to be thought of as subspecies. Those known as dent, flint, pop, sweet and flour corns are familiar to every one. One known as *Curagua* with toothed leaf edges, one with very hairy leaves known as *hirta* and one in which each seed is covered with husks or glumes known as *tunicata* are not so common. These varieties are our heritage from the aboriginal inhabitants, for each was known and cultivated some-

FIG. 9. A COMMON REVERSION HAVING SEEDS ON THE TASSEL.

where on the continent before the arrival of Europeans. They are considered by botanists as one species. The wild relative teosinte has been thought to be not only a distinct species but a member of a different genus. There is good evidence, however, that there is not a much greater difference between teosinte and the maize nearest like it than there is between a number of the most distinct maize varieties. These facts make it reasonable to suppose that both types arose from a common ancestor slightly different from each.

Teosinte and maize belong to the tribe Maydeæ, a division of the Gramineæ or true grasses. Our final problem is to connect the steps in the evolution of maize that distinguish it from the more typical grasses and if possible to picture the restored original form. The data from which one can do this come from observations of thousands of crosses between the different maize varieties.

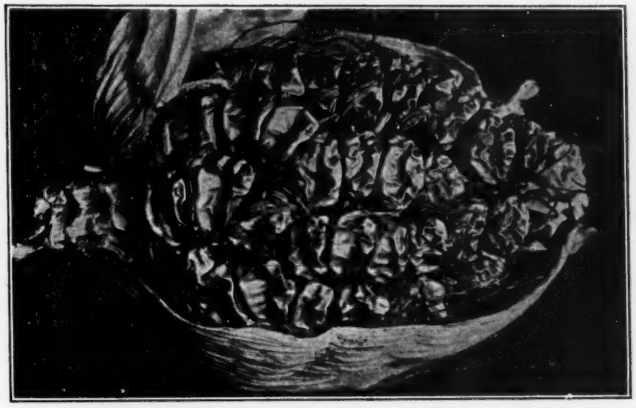

FIG. 10. A RARE REVERSION TO PERFECT FLOWERS.
Note the stamens around the seeds.

Sweet corn is probably the most recent type. Sweet corns are simply dent, flint, pop and floury types that have lost the ability to mature starch grains. This is proved by crossing it with starchy kinds. For example, dent corns crossed with certain sweet corns produce flint types in the second hybrid generation. Starchiness is put into the hybrid by the dent variety and the latent flintiness of the sweet variety appears.

In the same way crossing indicates that as the pop or poplike varieties increased in size by numerous slight variations, the flint, the dent, and the floury kinds were produced through the correlation between the structure of the seeds and their size. This brings us back to a many-branched pop-like variety, examples of which are common enough to-day.

Most maize varieties have naked seeds, a feature unlike other grasses including teosinte. The remaining members of the family have

FIG. 11. TEOSINTE. (Photo by L. H. Smith.)

the seeds protected from animal marauders by husks or glumes. This is again a simplification caused by the loss of a character, as is proved by crossing the ordinary maize varieties with the variety *tunicata* in which the character still remains. This gives us a grass-like corn with each seed covered—a plant in many ways like teosinte. It still differs from it by but one important and several unimportant characters, and the difference can not be particularly significant, for maize and teosinte cross freely and give fertile hybrids.

The difference is this: The female or pistillate spike of maize, the part which we call the ear, consists apparently of several two-rowed

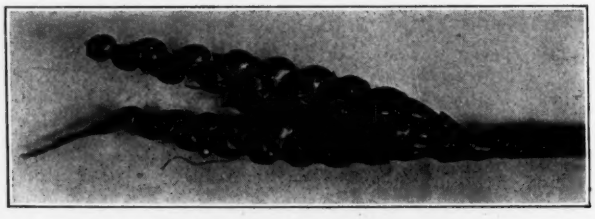

FIG. 12. A TEOSINTE-MAIZE HYBRID SHOWING THE DOMINANCE OF THE TEOSINTE CHARACTERS. (Photo by Webber.)

spikelets grown together; the same part in teosinte consists of bundles of distinct two-rowed spikelets with jointed axes. It takes two steps to bring maize to something like this condition. Ordinary maize varieties often produce individuals that have ears branched in much the same manner as the tassel or male spike. This is probably a reversion toward a former type. At least, pure varieties of this kind can be isolated. Furthermore it can be shown by crossing that the branched condition is due to a single hereditary character that has been lost by the cultivated kinds. The other step is the increase in number of rows, giving us the fine ears with from 18 to 24 rows that take the prizes in the agricultural shows. This feature is probably not due to the growing together of the spikelets. It is much more likely that increased number of parts came about through progressive variations, much as the increase of petals has brought the horticulturist so many double flowers. This type of variation is very common and still continues in maize, for the prize ears of the exhibitions contain many more rows than the more ancient little flints that were grown by the east coast red men.

The fact that but two essential variations, kinds that continue to occur, separate teosinte[2] from the maize nearest like it, combined with the fact that the two are fertile in crosses lead me to believe that the two plants are simply diverse types of the same polymorphic aggregation, although they may be called species if one desires.

Perhaps we should stop here and not follow the path of speculation to its uttermost limit; still there are two more backward steps indicated by studying the cultivated plant. The plant is monœcious; that is, the male organs and the female organs are borne in separate flowers, though both are found on the same plant. This condition is not uncommon among the grasses although it is not the primitive condition. The unique fact is that the female flowers that form the ears are borne on short branches in the axils of the leaves of the maize stalk, while the male flowers are borne in a terminal spike, the tassel. This method of flowering is not so peculiar if the ear branch is examined. The husks that surround the ear are merely the leaves of the lateral branch upon which the ear is borne as a terminal spike. The lateral branch has simply shortened. It is telescoped together until the distance between the nodes is sometimes not more than an eighth of an inch. It seems just to conclude from the number of these internodes that the ear branch was at one time as long as that portion of the main stalk above the ear, that the flower spikes of the ancestral plant were once more or less level topped, bringing them into a horizontal plane. What caused the change

[2] There are a large number of characters of less importance separating maize and teosinte that show that the two plants have developed along different lines after their separation from an ancestor more like both.

we do not know, but if the plants were already monœcious before the change, and such a variation occurred, it would have been likely to have continued to exist in competition with the parent form on account of the greater chance for perfect fertilization of the silks.

The last step in our history is to make ancestral maize a perfect flowered species; that is, a form in which each flower has both male and female organs. There is no question but that this was once the case. We know it by the characters possessed by the more ancient wild grasses and by the ease with which the plant reverts to the former condition. No one has isolated a race that breeds true to the older type, but every one who has raised corn has seen hundreds of tassels containing little seeds. It would seem that kindly external conditions alone are sufficient to bring back to the corn the memory of its old habit. When moisture is plentiful and the soil fertile, one can see these freaks by the hundreds in almost every field. The production of male flowers or their essential parts, the stamens, on the ears is much more rare, but it does occur.

Our history is complete. We can picture to ourselves the wild promaize growing on the plateaus of Mexico and Central America thousands of years ago. A towering prince of grasses it was, bearing its tiny seeds on loose spikes at the ends of the branches. Conditions changed. The perfect flowers separated into two kinds, bearing organs of the different sexes. A type with shortened side branches appeared, giving the seeds greater protection from feathered and furry enemies. This was probably the grain that some wise man among the forerunners of the Toltecs discovered and made the foundation of American agriculture. From that time forth cultivation made possible the selection of variations that would not have survived in the wild. Variation must have been plentiful, and our aboriginal corn breeders less foolish in agriculture than they were in commerce, as is demonstrated by the numerous varieties improved by long selection presented to the white man in return for a few paltry beads of colored glass.

THE UTILIZATION OF THE NITROGEN OF THE AIR

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A GERMAN geographer has estimated that the world contains 1,700 million people, and that they are increasing at the rate of twelve million a year. During each succeeding decade, therefore, provision must be made for feeding a new population greater than the present population of the United States. This demands an enormous, steadily growing increase in the world's output of agricultural products. How to provide for this increase is one of the largest material problems that confronts our generation and the generations to come. Many factors must contribute to its solution. New land must be brought under cultivation by a wider distribution of population, by increased facilities of transportation, by better utilization of the available water-supply through storage and irrigation. A larger yield per acre must be secured by improvement of the varieties of food-yielding plants through biological selection and breeding, through the adoption of more economical methods of farming, and especially through increasing and maintaining the fertility of the land by the scientific use of fertilizers in adequate amount.

This last aspect of the problem is the one with which this article is concerned. It is a vital part of the food problem, one which can not be eliminated by advances in any of the other directions just referred to; for plants can not live on water and air alone. They consist, to be sure, in largest proportion of compounds of carbon, hydrogen and oxygen; and they have the marvelous power of producing these compounds under the influence of sunlight from the carbon dioxide of the air and the water of the soil. But they contain also as essential constituents certain other elements, especially nitrogen, phosphorus and potassium, which they can not obtain from the air, which they must therefore extract from the soil. These elements are, however, present only in small quantity even in virgin soil; and they soon become exhausted through the harvesting of successive crops. It is therefore necessary, in the long run, to return to the soil the quantities of nitrogen, phosphorus and potassium that are contained in the vegetable products taken from it.

The sources from which we can obtain these three plant-foods cheaply and abundantly is so large a question that only one of them, nitrogen, will be here considered. Of the three this is by far the most expensive—by far the most difficult to obtain in sufficient quantity at low cost.

Before discussing the present and prospective sources of supply of useful compounds of this element, it should be mentioned that, though the consumption of these compounds in fertilizers exceeds all other uses of them, yet enormous quantities are required in other industries. Thus, the powerful modern explosives which have made practicable great engineering works, like the Panama Canal and the Hudson River tunnels, are all nitrogen compounds—made by the action of nitric acid on glycerin, cotton, or some other material. Most of the so-called coal-tar products, the artificial dyestuffs, drugs and perfumes, are also prepared from the substances distilled out of the tar by first treating these substances with nitric acid. Ammonia, too, a compound of nitrogen with hydrogen, is used in large quantity in refrigerating plants and in various chemical industries.

Up to a few years ago, there were only two important commercial sources of nitrogen-compounds—the great natural deposits of sodium nitrate (the so-called Chili saltpeter) in Chili, Peru and Bolivia; and the crude ammonium sulfate obtained in the manufacture of gas and coke from coal. But the saltpeter deposits will, at the present rate of exploitation, become exhausted within a period variously estimated at from 30 to 100 years; and, in the meantime, owing to increased cost of production, the price of the saltpeter is steadily rising, thus restricting its availability as a fertilizer. The ammonia produced in gas and coke works is only a by-product; and the quantity of it can not of course be increased beyond that corresponding to the demand for the main products, gas and coke. The total quantity of ammonia thus produced is in fact entirely insufficient to furnish the nitrogen used in fertilizers; and by far the larger proportion of commercial nitrogen is still derived from the saltpeter deposits of South America.

The nitrogen from these sources costs to-day in American or European markets not far from 15 cents a pound—a price which is causing a nitrogen famine among the crops of the world; for the cost is too high to admit of spreading it in adequate quantity over the millions of acres of land under cultivation. This condition of things offers a challenge to the scientific investigator. For, though nitrogen is one of the commonest elements, forming as it does, four fifths of our atmosphere, yet we are drawing nearly all our nitrogen from South American mines or from gas works and are paying fifteen cents a pound to get it in a form available for plant life.

It might seem as if the problem of converting the nitrogen of the air into compounds that can be assimilated by plants was essentially a chemical one; but recent discoveries have opened also to the biologist a great field of investigation in this direction. For it has been found that, although the higher plants can not utilize directly the nitrogen of the atmosphere, there are certain common kinds of bacteria, which

make their homes on the roots of leguminous plants, such as the pea, bean and clover, which have the power of absorbing nitrogen from the air and of converting it within the roots of the plant into organic nitrogen compounds.

This discovery explains for the first time the fact long known to farmers that the richness of the soil can be increased by rotation of crops—a fact so extraordinary, till its explanation was understood, that one might well have wondered whether it was not one of the fallacious traditions which are so common among farmers and sailors. This increased fertility is now readily accounted for as follows. Suppose that a crop of wheat is first grown on a piece of land, and that thereby the nitrogen compounds in the soil are largely consumed in producing the nitrogen compounds contained in the grain. Suppose now that the next year the same land is planted with clover. As it grows, the bacteria referred to develop upon its roots, absorb nitrogen from the air, and store up in the roots an abundant supply of nitrogenous compounds. After the clover crop is harvested, these roots decay in the soil, yield up to it their nitrogen-content, which becomes available for the nourishment of a new wheat crop during the following year.

An interesting illustration of these considerations has been furnished within recent years by the vegetation of the island of Krakatoa. It will be remembered that this island was overwhelmed in the year 1883 by an eruption of its volcano, which destroyed all vegetation and buried the original soil beneath a thick layer of volcanic ashes. It might have been expected that this new soil of ashes, which was of course free from all nitrogenous organic matter, would not be able to support plant life; yet the island soon became covered with an abundant growth. This vegetation was found, however, to be of an unusual character, in that it consisted very largely of leguminous plants—that is, of those plants which, with the aid of bacteria, can take their nitrogen directly from the air.

These facts suggest that the problem of supplying plants with the nitrogen needed by them may ultimately be solved most simply and directly by the biologist. For through further study of the conditions determining the activities of different species of nitrogen-absorbing bacteria, considered in relation to the kind of crop, the character of the soil and other agricultural conditions, it may prove practicable, by inoculating the soil with the proper kind of bacteria and by treating it in such ways as will best regulate bacterial growth, to secure all the needed nitrogen from the air. Already, government agricultural stations are furnishing pure cultures of nitrogen-absorbing bacteria which have a limited value in the case of certain soils.

Until such a perfect solution of the problem can be worked out by

the biologist, we shall, however, be dependent on nitrogenous fertilizers; and one of the great tasks of the chemist is to cheapen such fertilizers by obtaining the nitrogen contained in them directly from the air. During the last ten years great progress has been made in this direction; and it remains to describe briefly, without entering into technical details, the general lines along which this problem has been successfully attacked.

Two kinds of processes have been developed. One of these has the object of producing nitric acid, a compound of water with one of the oxides of nitrogen. The other kind of process has for its object the production of ammonia, a compound of nitrogen and hydrogen. For use in a fertilizer the nitric acid, which is a liquid, or the ammonia, which is a gas, must of course be converted into a solid salt. This is most cheaply done by neutralizing the nitric acid with lime or the ammonia with sulfuric acid, yielding calcium nitrate or ammonium sulfate, respectively. Whether the nitrate or the ammonium salt is made the constituent of the fertilizer makes little difference; for, though plants directly assimilate the nitrogen only in the form of nitrate, yet there are always present in soils the so-called nitrifying bacteria, whose function it is to convert ammonium compounds into nitrates.

Nitric acid is a compound whose constituents, nitrogen, oxygen and water, are present in unlimited quantities in the air. The raw materials are available free of cost. The problem is therefore only to make them combine under economic conditions. The difficulty arises from the fact that nitrogen is an extremely stable substance; so that, instead of tending to form compounds with oxygen, the nitrogen oxides tend rather to break down into their elements, nitrogen and oxygen. Thus, scientific investigations have shown that if a mixture of these two gases in the best proportions is exposed to a temperature of 1500° centigrade, that is, to a white heat, only one third of one per cent. unites to form nitric oxide, however long the mixture be heated. But these investigations have also shown that while most compounds decompose with rise of temperature, this one, nitric oxide, becomes more stable, the higher the temperature. Thus at 3000° five per cent. of the mixture of nitrogen and oxygen will unite to form nitric oxide. To get a fair yield of our product we must therefore expose air to an enormously high temperature. But this isn't all; for we must cool off the gas without causing the nitric oxide which has been formed to break up again into nitrogen and oxygen. To do this, we must call to our aid another chemical principle, which is this: although the quantity of a product finally formed in a chemical process sometimes increases and sometimes (as in this case) decreases with falling temperature, yet the rate at which that product forms or decomposes always decreases very rapidly as the temperature is lowered. We must,

therefore, expose the air to a very high temperature and then very suddenly cool it to a temperature so low that the nitrogen oxide already formed does not decompose at an appreciable rate.

These conditions have been practically realized in only one way—by causing an electric discharge, similar to that in an ordinary arc lamp, to take place in air. The temperature of the arc is enormously high, but the air just outside of it is comparatively cool; so that any nitrogen oxide formed at the boundaries of the arc mixes at once with the colder air and thus escapes decomposition. The excess of air containing the oxides of nitrogen is then passed into towers filled with quartz over which water is trickling, whereby nitric acid is formed.

It is not necessary to enter further into details; for these are the essential features of the commercial process for the manufacture of nitric acid which is now being carried out on a large scale at Notodden in Norway. Aside from the cost of installing and maintaining the electrical and absorbing apparatus, the only large expense involved in the process is the cost of power used in producing the electric discharge. The works must therefore be located where water-power is obtainable at the lowest possible cost; and Norway was naturally chosen as the seat of the industry in Europe. The saltpeter factories there are already utilizing 200,000 horse-power; and thousands of tons of their product have been shipped to this country, for use in fertilizing the fruit orchards of California and the sugar plantations of Hawaii.

Almost simultaneously with this process for the manufacture of nitrate there is being developed a process for the artificial production of ammonia, its competitor in the fertilizer field. The aim is to produce this compound also from its elementary constitutents, nitrogen and hydrogen. Nearly pure nitrogen can now be obtained cheaply from the air by a commercial process which up to twenty years ago had been carried out only on the smallest laboratory scale; namely, by liquefying air with the aid of a liquid-air machine, and then distilling the mixture of nitrogen and oxygen, much as a mixture of alcohol and water is distilled in the rectification of spirit. The nitrogen, having a much lower boiling-point, passes off first, yielding a gas containing less than half a per cent. of oxygen, which can readily be removed from it by chemical means. Pure hydrogen can be obtained cheaply by the decomposition of water in two or three different ways. The raw materials needed for the production of ammonia, although not costless like the air and water used in making nitric acid, are therefore obtainable at low cost; and the main problem again consists in finding a practical way of causing them to combine.

It is a curious fact that difficulties are met with here which are just the reverse of those encountered in the synthesis of nitric acid. Ammonia is a compound on which temperature has the opposite effect:

instead of forming in larger proportion as the temperature is raised, it forms in smaller proportion; thus, if a mixture of nitrogen and hydrogen be heated for a long time to 800° centigrade, only one hundredth of one per cent. of ammonia forms, while it can be calculated that at 400° one half of one per cent. of ammonia must finally result. We ought therefore to work at as low a temperature as possible; but we then meet the difficulty that the rate of combination becomes extremely slow. Thus, owing to the extreme inertness of nitrogen, no detectable quantity of ammonia is produced, even when nitrogen and hydrogen are heated together for several hours at 400°. When, however, it is known that a chemical change tends to take place in a certain direction and when the only difficulty is that it is going on too slowly, there is always a reasonable hope of overcoming this difficulty; for we know that chemical changes are often greatly accelerated by mere contact with suitable solid substances. Such substances are called catalyzers, and Professor Wilhelm Ostwald, one of Germany's distinguished scientists, predicted a dozen years ago that the great advances in the chemical industries within the next few decades would be made through the more extensive employment of catalytic processes. This prediction has found one of its many fulfilments in the commercial development of the method for the production of ammonia here under consideration. For after many years' investigation, certain metals have been found which cause a rapid combination of nitrogen and hydrogen even at comparatively low temperatures. The first metal that was found to have this power in a marked degree was osmium, a metal similar to platinum. As the total quantity of this element in our possession is estimated to be 200 pounds, and as it is valued at about $1,000 a pound, this discovery was hardly a practical one. Later it was found, however, that under special conditions some of the commoner metals, such as uranium, manganese, and even iron, when extremely pure, can be made to serve the purpose. Without entering into further details, it may be stated that a satisfactory yield of ammonia can be attained by carefully purifying the hydrogen and nitrogen gases, by highly compressing them (up to 50 or 100 atmospheres) and then passing the compressed gases slowly over one of these metals at 500–600°; and that a large factory for the manufacture of ammonia by this process is now being erected in Germany.

Certain other chemical processes for the fixation of atmospheric nitrogen, less direct than those already described, but nevertheless commercially practicable, have also been developed and put into operation within the past ten years. There is therefore little doubt that from these sources a large additional supply of nitrogen-compounds will soon be available and that their cost will be gradually lowered. To the vital problem of feeding the human race the chemist is therefore making an important contribution.

THE LABORATORY METHOD AND HIGH SCHOOL EFFICIENCY

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IT is a striking fact that for twenty years there has been *no increase in the percentage of pupils who complete a high school course.* In the period between 1900 and 1910, the number of pupils in public high schools in the United States has increased over 76 per cent. (from 519,251 to 915,061).[1] During this same period the number of high school teachers who teach these pupils has increased over 100 per cent. (from 20,372 to 41,667). The number and value of high school properties has increased proportionately during this period, including improvement in the quality and quantity of facilities for work in libraries, laboratories, gymnasia, etc. But for twenty years, approximately twelve per cent. of the enrollment of the high schools has been graduated. Regardless of the increase in facilities, and of an increase in teaching force, which is one third greater than the increase in the number of pupils, and of an assumed increase in the relative efficiency of this teaching force, and regardless of the increased public belief in secondary education, there has been no increase in twenty years in the percentage of high school pupils that take a full high school course. The fact that they begin the work indicates clearly that some one in control regards it as worth while for some reason for these pupils to engage upon the work of the secondary schools, though they may at the outset expect to do but one or a few years of the work. But the fact that approximately 88 per cent. do not complete a course indicates that most of those who thought it worth while to enter the high school, for some or many reasons do not find it possible or perhaps not worth while to follow out the course, even if at the outset they intend doing so.

Failure to carry school work is one prominent factor in the elimination of pupils from school, though doubtless the content of the curriculum, and social and economic conditions may often be determining or contributing factors. In one large high school 432 pupils entered the freshmen class in the autumn of 1909. Of these 432, 338 left school before completing the third semester, thus leaving 94 of the original 432 in school. Of those who left, 124 made no passing credit in the school and 121 others failed to receive passing credits in 43 per cent. of the subjects which they took. The remaining 93 pupils who left school made average grades above 80 per cent. (75 being the passing grade in

[1] Ann. Rep. U. S. Com. of Ed., 1911, p. 9.

this school) though the pupils failed to secure credit in 22 per cent. of their subjects. There were, therefore, 245 of the 338 pupils who had a percentage of failure from nearly half to all of their subjects, and 93 pupils who failed in 22 per cent. of their work. The 94 pupils who remained in school failed to receive credit in slightly less than 5 per cent. of their subjects.

It seems possible that this case is more striking than would usually appear from such investigations since the problems associated with this particular school may be peculiarly difficult.

In a careful study made by Mr. G. R. Johnson, of St. Louis, and covering records from twelve high schools with a total number of 18,926 pupils, he finds that approximately 90 per cent. of those pupils who were failing in their work left school, while but ten per cent. of those who were making 90 per cent. or better in their work left school. This percentage of those who failed and left school remains almost constant throughout the four years, with the exception that in the Chicago and Kansas City schools rather a larger percentage of the failures drop out in the earlier years than in the later years, while in the smaller schools the percentage of dropping out of those who fail remains about the same throughout the whole high school course.

Doubtless the compulsory attendance law and the sixteen-year labor law often are factors in continuing for a time the attendance of pupils who do poorly, and that with the close of the sixteenth year economic and social necessity takes many pupils out of school. But we must note the fact that the percentage of failures who leave school remains almost the same for all the years of the high school. Possibly the termination of the period when pupils *must* attend school may operate to relieve those who are failing, from the necessity of further attendance in an institution in which they do not "make good."

School methods (of dealing day by day with the series of topics that make up a given study) *are often contributing causes to the failures which lead pupils to leave school.*

The present situation is interesting. In the elementary schools from which these pupils have come to the high school, the school day runs from 8:30 or 9:00 o'clock to 3:30 or 4:00 o'clock and the greater part of all study is done during school hours, under direct or indirect supervision of the teacher. The teacher is present to correct any misunderstandings in assignments, to give a directing question or suggestion, or to quicken the endeavor, when such is needed. The work of one year is fairly well connected with that of the preceding years and partially new and partially old ground is covered each year. In the high school, particularly, in the first year, the subjects of study are largely or wholly new, often so new as to constitute fields quite unknown to the pupils. Even when some of the subjects are not new, we

have a larger change than occurred between any two elementary grades. Pupils in a given subject go to the special room of the teacher for their recitations, recite and receive their assignment and then go to another class room for another subject, or return to their assembly room or to their homes with their assigned work for the next day. The teacher in the elementary school ordinarily meets the pupils of a given grade for most or all of their work, and knows them as they appear in all their work. In high school each teacher is especially interested in one or a few subjects and this one or few are the only ones in which the teacher knows his pupils. In the elementary schools the teacher usually stands as representative of one *grade of pupils.* In the high school the teacher usually stands as representative of *a subject.*

Not only does the first-year high school student encounter a new content of subject matter, but usually a new kind of school day. Many high schools begin work at 8:30 or 9:00 o'clock and close at 1:00 or 1:30 or 2:00 o'clock. In many high schools all of the hours in school are occupied in recitation or laboratory work, all individual study or assignments being done away from school.

The conditions for home study present all the possible variations, but most home study must be done under discursive influences—a little study, a little conversation about irrelevant matter, an intermittent discontinuance for small household duties, a prolonged intermission for recreation, with the half-consciousness of wrong-doing because of unfinished and overhanging lessons, even interrupted sleep because of a number of unfinished tasks, a final effort to secure categorically such facts regarding the assignment as are essential to enable the pupil to meet the teacher, a consciousness of incompleteness of preparation and a hope that, if called upon at all, the call may come for the facts that are in the pupil's meager store. Often the pupil's own initiative to home study must be supplemented by commands or entreaties from parents, and sometimes parents must do pupil's work for them, under penalty of family chagrin due to impending failure of the child. In most cases poor habits of study and an essentially immoral attitude toward study result from purported home study, though some pupils of good ability and strong individuality may do quite effective or superior work through home study. The habit of dawdling, waste of time in getting to work, wondering whether the work really must be done, whether a lexicon, cyclopedia, or parental answer to questions may not be found, leaves an entirely improper attitude toward real study. Sham work, at first as a makeshift, later becomes the only kind of which some individuals are capable.

Some important experiments have been made to determine the relative value of directed and individual class-room study.

It has seemed to several teachers to be worth while to see if more

carefully directed class-room study, less so-called recitation and less home work might not yield better results.

Experiments *in mathematics* have been carried on by Mr. Ernest R. Breslich, of the University High School (Chicago), Department of Mathematics. At the outset Mr. Breslich found that some pupils who did poorly on their assigned work did not understand the suggestions that had been given regarding good ways for undertaking the home work. Parents insisted that the assignments made were impossible, whereas for one reason or another the pupils had failed to get essential suggestions regarding the assignment. Even with assignments clearly understood certain habits of home study which did not exist had been assumed. A series of visits to other classes showed similar conditions. Pupils reported poor results from their home study, various excuses or no excuses being offered. The teacher explained away the pupil's difficulties and, in most cases, the pretense of having the work done at home was continued.

To ascertain the ways in which the members of one class attack their work, Mr. Breslich assigned a lesson, taking unusual care to make clear all phases of the assignment. The class was then told that the next fifteen minutes would be given to studying the lesson assigned. All pupils were slow in beginning the work and some occupied all of the fifteen minutes in getting ready to go to work. Some who ordinarily came to class with well-prepared lessons looked about to see how others were undertaking the work, and followed them. Few really accomplished anything in the fifteen minutes.

To investigate more carefully these individual habits of study, Mr. Breslich told his classes that at a certain hour each day the class room would be open to students who had difficulty with assignments or wished to make up back work, and good use was made of this opportunity. The teacher passed about among the pupils as they worked, making suggestions, but rarely answering questions directly.

It was then decided to make more prolonged trial of this supervised study with all members of one class. In one section of the class no home work was assigned and in the other section home work was assigned and in the usual way. The two sections had the same work. Both spent fourteen lessons on simultaneous linear equations, at the end of which the same test was given to both sections. The relative standings in grades which these two sections received upon the same examination, at the close of the preceding semester in mathematics, that is prior to beginning these experiments, are: Section A, average 81.4; Section B, average 79.4, B being slightly weaker than A. In Section B 5.9 of the class had failed in the preceding semester and none in Section A.

Section A was given home work with no class room supervised

study. Section B was given supervised study and no home work. Upon the test following the fourteen lessons their standings were:

Section A—with home work and no supervised study averaged 62.8, with 50 per cent. receiving the failure mark.

Section B—with supervised study and no home work averaged 65.5, with 31.2 receiving the failure mark. It is to be noted that Section B, a somewhat weaker section, surpassed Section A, and that its lower number of failures indicates that the poorer pupils profited most from the supervised study. Section A reported an average of 1¼ hours spent on each lesson, while in Section B the actual time of class work was 36 minutes per day. Section B solved an average of two problems more to each pupil than did Section A. With the supervised class work as a basis, too much time was spent on the home assignments. Section B worked slowly during the first three lessons, but with the development of independence and confidence they soon worked rapidly. The interest and pleasure of Section B, some of whom had failed in the preceding semester, were noticeable.

In the following topic to which six lessons were given, the methods were reversed, Section A being given supervised class-room work and Section B home assignments, and class recitations. At the close of this series of lessons the same test was given both sections with the result that Section A with supervised work and no home work averaged 77.5, and Section B with home work and recitations averaged 86.4.

12.5 per cent. of Section A failed on the test and 5.7 of Section B failed.

31.2 per cent. of Section A secured a mark of A, and 52.9 per cent. of B secured the A mark.

This seems to show that the pupils in Section B, by means of their previous fourteen supervised lessons, had learned enough about independent study to enable them to do their home work in such a way that Section A even under supervision did not surpass Section B in six lessons. The ability of Section B, gained under supervision, persisted in home study through six following lessons.

In the Detroit Central High School a different plan has been followed in some *experiments in algebra and Latin.* Principal David McKenzie writes:

> We have experimented somewhat with a plan to give additional direction to the weaker pupils of the ninth grade. I cite the two cases of first course in Algebra and Latin. At the end of ten weeks all pupils who were marked failing in these subjects were grouped together for special work in addition to their regular recitation periods. They were given twenty lessons each, on the ground covered during a period of six or seven weeks. Each pupil was treated as a pathological subject. In the final test they were marked as follows:

LATIN

Total number of pupils	15
Number marked "Excellent"	1
Number marked "Good"	6
Number marked "Fair"	3
Number marked "Weak"	1
Number "Not passed"	3
Number "Left"	1

ALGEBRA

Total number of pupils	20
Number marked "Excellent"	2
Number marked "Good"	4
Number marked "Fair"	3
Number marked "Weak"	5
Number marked "Not passed"	3
Number marked "Left"	3

It is plainly evident that a large number of ninth grade pupils need greater direction than they receive at present, and I am convinced that we must resort to some plan to give them this additional help, if we are to eliminate excessive mortality in this grade.

The *double period plan* is in use in many schools. In the Joilet Township High School for some years two periods per day, ten hours per week in all, have been given to all science work, manual training, domestic science and mechanical drawing, this period being used both for study and recitation. This school has also used this plan with beginning algebra, beginning geometry and beginning history. In Joilet the consensus of opinion of teachers is that the plan is successful. Principal J. Stanley Brown states that by such a scheme "the percentage of failures may be reduced to a minimum, and that is a compensation for the slight increase in teaching force and extra amount of money spent for teaching."

At Murphrysboro, Ill., an experiment (in *manual training*) has been under way which, while not bearing directly upon our question, has a collateral bearing upon it by indicating that even single periods and more prolonged periods of class instruction may sometimes be used in such ways as to make the shorter and not the longer period desirable, though doubtless longer periods usually are desirable.

A small class of boys in manual training was divided, one section being given single periods for this work, the other the same number of double periods. The principal, Mr. G. J. Koons, stated that the single period pupils were not above the double-period boys in their general class standing nor in ability. All were given piece work and records were kept of the hours used by each boy in completing each piece of work. Eleven pieces of work were completed by each pupil. The single period pupils used approximately 25 per cent. less time on an average for each piece than the double-period boys, and on the test given to all

at the close of the work, the single period pupils averaged 7 per cent. above the double-period pupils. This experiment suggests a possible waste of time in longer periods, possibly lack of readiness in attacking work, of attention and high tension of effort throughout the period. It is well known that appreciation of relative shortness of time available usually results in higher alertness, readiness of attack, higher tone and more constant prosecution of the work in hand. It must be kept in mind that the Murphrysboro experiment involves a small number of pupils and withal may be more of a suggestion of method than of the value of any particular length of period given to a study. Most teachers who have tried class-room directed study find double periods, part for study and part for general discussion, most effective.

Variations of the above experiment are under way in other schools.

Throughout the whole United States there has been a significant attempt to introduce courses in *general science* into the first year of the high school. While in different schools these courses vary largely in their content, length and in many details of method, they agree in their purpose of being less formal, less rigid and abstract than the highly differentiated sciences, and in selecting and treating topics in science in such ways that the pupils think through these topics with good methods of thinking and with a knowledge content that appeals to the pupils as being worth while. The dominant method is that of class study of real things and real situations. An active attempt is made to secure individual experimentation or individual study from every pupil. The whole general science movement is an attempt to secure a scientific method of work, upon concrete problems, the significance of which appeals to the worker. We have been putting first-year pupils into formal sciences which were beautifully organized and orderly, possibly even elementary from the point of view of the adult science and the research student, but which are an abstract field to the pupil who has not been led to rationalize the common phenomena of his surroundings. This general science course has met a splendid response and its method has resulted in more effective work in subjects other than science during the first year and in the sciences in the following years. It is stated by teachers and principals that where significant laboratory courses in general science are given, fewer pupils fail in their work, more remain in school in the second year, and there is a much larger demand in subsequent years for courses that utilize laboratory methods, similar to those of general science courses. The method and significant content of the general science course seems to prepare in ability to work and in desire to work in other laboratory courses. My own observation leads me to conclude that the oft-made statement that pupils are naturally averse to work, is much exaggerated. If properly guided to independent, purposeful study, really significant work becomes a pleasure to most pupils.

General science is an attempt to get back to the valuable parts of the natural history of our fathers, the purposeful, dynamic, thoughtful but elementary interpretation of common significant problems. The kind of interpretation which physiography promised to give when it first came into secondary schools and which physiography may still serve to unify better perhaps than any other single branch of science.

The more fully directed study in general science and in other laboratory sciences presents an opportunity for individual, first-hand study of concrete things for experiment and interpretation of phenomena. But, as is true in other high school subjects, it is wasteful for the science teacher merely to assure himself that the pupils and materials are enclosed within the same room. Science in which we boast of concrete studies, of the laboratory method and of the possible significance of content that is unsurpassed, has sometimes become as formal in its home assignments as unlikely of achievement, its recitations as free from individual dynamic activity as any other subjects. It as well as the other subjects needs to be revived by use of its own concrete laboratory method. Laboratory teaching in science or other subjects may rise to the highest level of excellence or may descend to a meaningless mechanical manipulation that is deadening. But it is believed that the laboratory method offers us an important method greatly needed in all our high school subjects, most seriously needed in the first years of the high school.

It must be obvious that if such methods of high school work as suggested by the experiments cited above are used, *some important changes must be effected.* Most important is wider recognition of real teaching, real development of pupil-power, as compared with assigning and hearing lessons and telling facts to pupils, in case they have not understood them. Recitations and class discussions and home assignments should not be wholly omitted, but these may profitably be much reduced. Then, when teachers direct their pupils in individual study of real situations, assignments may be expected to become more appropriate, more carefully planned, less frequently made at the close of the period as the class is starting from the room. The assignment is a highly important part of the period's work, and it is an educational misdemeanor to make an incomprehensible assignment.

The extension of these methods of study would help to eliminate some of the abuses of the ordinary class room recitation. With directed individual study, each would have fuller opportunity for work, and each must learn to work independently. It does not follow that all general discussion should be omitted, but in directed work there are ample opportunities for general discussions. Nor does it follow that no home work should be assigned.

A more intimate interest in each pupil is possible through class-

room study. The ordinary assignment of home work and class-room recitation method tends to reduce all the class to a base level. Class-room study enables the teacher better to teach both weak and strong pupil, to his highest efficiency. The ordinary class recitation method —a sort of vermiform appendix on our educational system—often consists either in allowing the best students to do the work or in having them sit idly by, developing habits of low tension while the teacher attempts to pull up the weaker ones to a fair understanding of the point at hand. It requires a higher order of ability to teach genius than mediocrity, and our present class-room methods often ignore genius, through an illy balanced sense of duty to the mediocre, or may neglect the majority in the interests of the few brighter pupils. Well-balanced study should enable the teacher to stimulate all to a high degree of effort.

Class-room study means a longer school day and more teaching force or longer hours for the present teaching force. The school day should be longer. Germany has approximately thirty school hours per week to our approximate twenty hours per week in secondary schools.

Almost all high-school work should be done at school in school hours under guidance of teachers. Less assigned home work will mean less carrying of responsibility for school duties during the hours at home when often such responsibilities can not be met and under conditions which often foster ineffective habits of study. There will always remain plenty of good home work; good reading, some assignment, upon work in line with school work; but our pupils should no more carry home with them the larger burden of their school work than a good business man should take home with him his major business duties.

The longer school day is not to be feared, but welcomed, if by means of it adequate time for proper study is secured. We have cheapened our schools by shortening them. Even longer hours for teachers, the time being given to more prolonged and more effective teaching in a reduced number of classes, is not undesirable, if by means of these longer hours more effective teaching and less wreckage through failure in high school may be secured.

HOW EUROPEAN AGRICULTURE IS FINANCED

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THE American farmer is ahead of the European in many things, particularly in the use of labor-saving machinery. But in the application of business principles in their financial operations, the European farmers have perfected systems that are in advance of anything yet attempted in America. This has been largely brought about by the force of circumstances necessitating an economic transformation. During the last century the competition of new countries with immense areas of virgin soil flooded the European markets with agricultural products and forced the European farmers to reorganize their business methods.

As a result they have organized to make available abundant credit at low rates of interest and on favorable terms of repayment. By credit, it is not meant that the farmer gets everything he buys on time without paying anything on it, and that he is in debt on every hand, but just the reverse. It means he has money available at all times, so that he may pay cash for everything he buys, thus getting the benefit of the lowest cash prices and discounts. His credit is at the bank and not at the store, and through the bank he gets the loans that he needs at rates of interest just as low and in many cases lower than secured by other industrial enterprises, no difference how large or how much business they do. But to accomplish this the farmers have had to take a hand in the banking business themselves. They have organized on a cooperative basis to secure the credit they need and to supervise its distribution rather than leaving it to private interests to supply the same. By so doing they have reduced rates of interest, lengthened the time for which loans are made, provided for the repayment of loans by annual installments, and they keep the money in the rural districts and prevent its accumulation in the large cities.

Sources of Capital

The sources from which the capital is drawn that is thus made available to the use of the farmers may be classified under three heads: (1) subvention from the government, (2) savings deposits of the farmers and rural population, (3) from the sale of bonds secured by mortgages on farm land.

The relation of the governments in furnishing agricultural credit has varied greatly. In France the rural banks have been established for the most part on funds advanced by the government without interest. This policy was begun in 1894 and in 1910 the working capital at the disposal of the rural banks which had state aid amounted to 71 million francs (between 14 and 15 million dollars), of which 40 million francs had been advanced by the government. In Austria the provincial governments have actively assisted in the establishment of rural banks to furnish credit for farmers and have advanced loans without interest to them. In Germany the government has indirectly aided the rural banks by establishing central banks founded on capital advanced by the government, in most cases at 3 per cent. interest. The central banks in turn furnish credit to the rural banks and the rural banks to the farmer. The Prussian Central Bank at Berlin now has a capital of 75,000,000 marks from the Prussian government. However, its business is not confined to agricultural banks, but is open to all kinds of industrial cooperative associations. It receives deposits and makes loans to the cooperative banks throughout the kingdom of Prussia, and serves as a compensating medium between the different cooperative institutions. For example, if a rural bank has large deposits and a surplus of funds, it deposits them in a central bank to be loaned to some other bank in need of funds.

The desirability of government subvention is a disputed point, and in Germany which has the best developed system of agricultural credit in the world, many are opposed to it as being entirely unnecessary and think that a better system can be developed without it.

The second source of capital, savings and deposits of the farmers and rural population, is the most important. It has the advantage of developing the habit of saving among all classes in the country and it keeps the money in the rural districts in which it is earned. In Germany alone there are over 16,000 rural savings and loan banks with one and one half million members and deposits of over $250,000,000. Instead of being deposited in savings banks to be loaned out in the cities, as is the case in America, or deposited in postoffice savings banks to be loaned to city banks, the money is kept in the rural districts and loaned out at a rate of interest that the farmer can use it to advantage.

The third source of capital, obtained by the sale of bonds secured by mortgages on farm lands, was the first form of cooperative agricultural credit established in Europe and was begun in Germany in 1770. Its most rapid development, however, has been within the last thirty years, and at the present time the German farmers have over $1,000,000,000 borrowed in this way, none of it costing them more than 4 per cent. interest and in some cases it is as low as 3 per cent.

Agricultural Credit in the Province of Saxony

The agricultural credit institutions of the province of Saxony in the kingdom of Prussia are as highly developed as in any place in Europe and are typical of the German system. The province of Saxony lies in central Germany, contains an area of 9,750 square miles and in 1910 had a population of 3,088,000, equal to 315.7 per square mile. The largest cities in the province are Halle and Magdeburg with 180,000 and 280,000 population, respectively. It is the heart of the sugar-beet district of Germany and the richest agricultural section of the entire empire. It contains 97,000 farms of over five acres in size. The estimated worth of the land per acre is $300 (for the whole of Germany it is $150 per acre). It is a typical agricultural province, in which the most intensive systems of agriculture have been developed, necessitating the investment of a large working capital per acre, which has been made available through the development of agricultural credit institutions.

These may be divided into two classes: (1) the institutions furnishing real credit, that is, loans secured on farm mortgages made through the public land mortgage bank—the so-called *Landschaft* of the province of Saxony, (2) institutions for furnishing personal credit, that is, working capital on short time loans and on personal security which is provided through the farmers' cooperative banks.

The Land Mortgage Association

The German Land Mortgage Association (*Landschaft*) was first established in 1770 by the nobility of eastern Germany, with the approval of Frederick the Great, for the purpose of securing loans on their farm real estates. Instead of borrowing individually they organized an association and issued a common mortgage bond against all of the real estate owned by the members of the association. Furthermore, the management of the association was under the direct control of the government and the officers were quasi-public officials. Other similar institutions were soon established, but confined their members to the nobility and large landowners. However, the results secured were so satisfactory, the rates of interest so low and terms of the loans so favorable that the plan extended and the farmers of the middle classes organized in a similar manner.

The province of Saxony, in which the farmers of the middle class predominate, did not organize a land mortgage association until 1864. A few years later came the war between France and Prussia (stopping industrial development) so that in reality the association did not make real progress before 1880. To-day the total mortgage indebtedness of the province is 830,000,000 marks, and over 220,000,000 marks of these loans have been made through the Provincial Land Mortgage

Association. The proportion of the loans made through the association is constantly increasing and within the last six months they have increased 10,000,000 marks, but the time probably will never come when all of the outstanding mortgage loans will be made through the land mortgage associations, as in many cases mortgages are given by members of families in settlements of estates, loans are made within families and through other private interests, so that in no case is it likely that over two thirds of the mortgage indebtedness of a province will be made through a public credit institution.

The Business of the Land Mortgage Association

The Land Mortgage Association of the province of Saxony, which is typical of all other similar institutions in Germany, is a cooperative union of the landowners of this province for the purpose of securing loans for its members on their land by issuing bonds (*Pfandbriefen*) against the same. The association is not a stock company. No profits are declared to individuals, but go to the reserve funds of the association. Any one may become a member who is a landowner in the province and pays a land tax of at least 90 marks per year, which means owning from 10 to 25 acres of land, depending upon its value.

The articles of the incorporation for the association were approved by the Prussian government and the oversight of the business is under the direction of the Minister of Agriculture of the kingdom of Prussia. The association is independent to conduct its own affairs and to elect its own officers, but the election of the higher officers must be approved by the government. A farmer wanting to borrow through the association makes his application. After examination of the title of his farm and finding it satisfactory he has the privilege of borrowing to two thirds the assessed value of his farm for taxation by giving first mortgage to the association for the amount he borrows. The association does not have the money on hand to make the loan, but secures the same, not by selling the mortgage, but by issuing what is known as a *Pfandbrief* or mortgage bond of equal amount to the mortgage and selling the bond. There are several features of the *Pfandbrief* that are characteristic. First, it is not secured alone by the mortgage of the farmer for whom it was issued but by all the mortgages and property of the land mortgage association. Second, it is transferable without endorsement at any time and is an impersonal security payable to bearer. Third, it is not a bond in the sense that it runs for a definite length of time, for there is no fixed time at which it matures. Fourth, the holder does not have the right to demand payment of the face of the bond—that is, to call in the loan—but the issuer—the land mortgage association—has the privilege of paying it at any time. For example, the bond may be called in and paid six months after it is

issued or fifty years, at the pleasure of the land mortgage association. But under no conditions is the amount of bonds outstanding permitted to exceed the amount of mortgages held by the land mortgage association.

The business of the land mortgage associations has been done so conservatively that their bonds are regarded as the very best of security and are favorite investments for trust funds, savings banks and any capital seeking a perfectly safe investment negotiable at all times. In fact, these bonds sell next to government bonds, and in case of war, or even threatened war, they sell better. The government may be overthrown or compelled to suspend payment of interest, but the farm real estate that secures the bonds can not lose its value.

The rate of interest the bonds bear is 3 per cent., 3½ or 4 per cent., at the option of the farmer securing the loan, but the price at which they sell depends upon the condition of the money market. At the present time (July 1, 1912) 3 per cent. province of Saxony *Pfandbriefen* are selling at 81.00, 3½ per cent. at 90 and 4 per cent. at 99.80, while the 4 per cent. national bonds of Germany are selling at 100. In case a farmer borrowing $1,000 chooses a 3 per cent. interest rate and the 3 per cent. bond are only selling for 81.00, he gets only $810 and pays $30 per year interest, that is, 3 per cent. on the face of the bonds, and gives his note and mortgage for $1,000. But on the other hand, if 4 per cent. bonds are selling at par and he chooses a 4 per cent. loan, he gets his $1,000 in cash for his *Pfandbrief* and pays $40 per year interest and gives his mortgage and note for $1,000. It is always regarded as the best policy for the borrower to choose the class of bonds selling nearest par, unless they are selling above par, in which case the farmer securing the loan gets a premium over and above the amount of his liability and it is to his advantage to take such loans. When the bonds go above par they are called in and paid off by the farmers refunding their debts at lower rates of interest. Here comes the advantage that the farmers reserve for themselves in the privilege of paying off the bonds at will. Just such a thing happened when in the seventies the rate of interest advanced to 5 per cent., due to the scarcity of money and the enormous demand for it in building railroads on the continent and ten years later the rates of interest sank until 3 per cent. bonds sold close to par and the farmers rapidly paid off their loans made at the high rate of interest by using new bonds at the lower rate of interest and selling them to pay off the old ones.

Central Land Mortgage Association

In order to widen the market for the *Pfandbriefen* a central land mortgage association was established in Berlin in 1873. By this means it was thought to make them an international security and to give them

a larger market. The bonds of the central association are secured by all the mortgages of the provincial associations belonging to the central association. The results attained through the central association, however, have not fulfilled expectations. The *Pfandbriefen* in no considerable extent have found their way into the international money markets. The offering of them in such large quantities on the Berlin Bourse reduced the price below what they could be sold for in home markets through the local banks. Furthermore, there is a sentiment among investors buying bonds that as long as the provincial bond is equally as secure as the central they prefer to invest in *Pfandbriefen* of their own province. In the province of Saxony, with its 220,000,000 marks of *Pfandbriefen* outstanding, the director of the *Landschaft* estimates that 75 to 80 per cent. of the total amount invested in them is capital of the province of Saxony. So far as security was concerned, nothing was to be gained by consolidation into a central association, since the provincial association bonds are as secure as bonds can be made.

Of the total amount of bonds in circulation at the present time only about 10 per cent. of them are central association bonds. The latest statistics show that the provincial and central association of Prussia have the following amounts of outstanding bonds.

LAND MORTGAGE ASSOCIATION OF PRUSSIA

Association	Founded	Outstanding Bonds
East Prussia	1788	426,152,350 M.
West Prussia	1787	123,074,405 M.
New West Prussia	1861	186,278,210 M.
Berlin	1868	449,563,500 M.
Pomerania	1781	252,007,525 M.
New Pomerania	1871	19,006,900 M.
Posen	1857	301,525,300 M.
Silesia	1770	608,634,180 M.
Saxony	1864	126,675,600 M.
Celle	1790	15,579,100 M.
Hanover	1825	24,706,650 M.
Bremen	1826	10,360,425 M.
Westphalia	1877	74,554,300 M.
Central	1873	433,255,000 M.
Total		3,093,493,545 M.

Mark equals 23.8 cents.

AMORTIZATION OF LOANS

One of the most valuable features of the loans made through the land mortgage associations from the standpoint of the farmer is the gradual amortization through annual payments made with the interest. This is obligatory on the part of the borrower and usually is ½ per cent. to ¾ per cent. of the face value of the loan. In the land mortgage

association of the province of Saxony the amortization is ¾ per cent. per year. On a loan made at 4 per cent. is added the ¾ per cent. amortization and ¼ per cent. to cover the operating expenses of the association, making a total of 5 per cent., and by paying this amount annually for between forty to forty-five years the loan will be paid off. The farmer in the meantime also has the privilege of paying it all or in part at any time. After the loan has been made the rate of interest can not be raised or the loan called in, so if the farmer has secured his loan at a low rate of interest he can carry it until it has been amortized by his annual payments. The Saxon farmers who in the nineties borrowed at 3 per cent. and got par for their bonds are relishing this feature now that the rate of interest has advanced to 4 per cent.

However, many of the better farmers make no attempt to pay off their loans any faster than is required through the annual amortization payment, finding that they can get their credit cheaper in this way than any other and can make more interest on the money used in their business than they have to pay for it. The association also has the provision that when 10 per cent. of the original loan has been paid an additional loan can be made and in this way a farmer can continue to carry indefinitely practically the same amount of loan on his property if he finds it advantageous to do so. The average length of time loans run in Saxony is about twenty-five years.

By this method the farmer gets all the advantages of the money market if money is tight—the rate of interest goes up and the price of the *Pfandbriefen* go down when money is abundant and interest rates low the price of *Pfandbriefen* go up. The farmer through his bank watches the money market and takes advantage of the low points in interest rates to secure his loan, and once made he is safe from having his loan called in or his interest rate raised.

Decentralizing the Business

A practical point in the operation of such a business is to make it as convenient as possible for the farmer to do business with the land mortgage association. The province of Saxony is a territory nearly 100 miles square and the association is located in Halle, a relatively large city. For all of the farmers to come to the central association to negotiate their loans would be impracticable and would diminish the business very much. This problem has been solved by dividing the province into districts 10 to 15 miles square and in each district is a local officer of the association elected by the members in their annual meeting. This officer assists the members in getting their loans, sends in their applications, gives information concerning the association and looks after the business in his district. When property is appraised for loans, he is chairman of the committee making the appraisement.

When interest is not paid or a member is neglecting his farm, the local deputy, as he is called, serves as the medium between the association and the delinquent member. In this way the advantages and economy of a centralized organization and at the same time the benefits of a decentralized association—that is, one close to the individual farmer—are secured.

Personal Credit

While the land mortgage association is sufficient to provide the long-time credit that is needed by the landowner, it does not suffice to furnish the short-time loans that are needed to supply working capital, to buy seeds, fertilizers, livestock to be fattened, to pay for labor to grow crops and such operations as require capital for six to nine months. To the farm renter or any farmer who does not own land, the land mortgage association has nothing to offer.

To meet this need the rural banks have been established. The work of this class of banks had its beginning particularly with William Raiffeissen among the peasant farmers of western Germany about the middle of the last century. Raiffeissen saw the dire straits of the small farmers who were without credit and at the mercy of the usurer. He began by establishing cooperative associations to do their own banking, and there were four fundamental principles that he insisted upon that have been retained in the true Raiffeissen banks of the present time. First, unlimited liability of the members. This was necessary in the beginning in order to get any credit at all. All the members were practically without means and the question of limited or unlimited liability was of little moment to them.

Second. A restricted area of operation for the bank. This was confined to the district in which the members were all personally acquainted with one another. In European farming it is customary, especially for the peasants, to live together in small villages and not on single farms as in America, so that the boundaries for the operation of the bank were generally confined to a single village.

Third. No dividends to members. A low rate of interest, usually 4 per cent., was paid on the capital stock each member had invested in the bank, but all profits made over that amount were set aside in a reserve fund.

Fourth. No salaried officers were employed in the banks except the bookkeeper. The management of the bank was made a matter of honor, the work to be done without any mercenary compensation. The business was done in the most democratic manner possible. Every member was given a voice and made to feel he was personally responsible for the success of the business. Loans were made for specific purposes, for example, to drain a field. The committee considered the

advisability of the proposed expenditure in making the loan, the members of the bank all knew the plan of the member and were interested in his success, because in case the member failed and was unable to repay his loan to the bank they would all be losers.

Raiffeissen did another thing that is of utmost importance in rural banking. He adjusted the loans of the bank to meet the needs of agriculture. The farmer needs a longer time loan than the merchant or manufacturer. City loans for three and four months do not fit the business of farming. With the farmer 6 to 9 months is the shortest time for which he needs a loan. The time from planting a crop till it is harvested and ready to market is at least six months. The city merchant will turn over his money four or five times during the year but the farmer only once, so that the rural banks must make the loans for longer periods than is customary in the city. In case of crop failure in bad seasons loans must be allowed for still longer periods and in Raiffeissen banks these provisions were made.

From their beginning in the Rhineland the Raiffeissen banks have spread not only over all rural Germany, but almost all rural Europe. They have been modified to meet local conditions but with it all have kept in view the purpose of serving the needs of the farmer.

In studying the agricultural banking or credit system of a country the condition of the individual farmer must be taken into consideration. A system applicable to peasant farmers with small holdings, such as are found in many parts of Europe, is not likely to offer much of value for American farmers. But in a section in which the average wealth and stand of the farmers is on the same level as in America, a system that is proving successful may afford some good lessons.

Rural Banks in the Province of Saxony

Such a section is to be found in the province of Saxony where the rural banks are splendidly organized and doing a business of $100,000,000 per year.

The first striking difference between these rural banks and the original Raiffeissen banks is that they are organized on a limited liability basis. The farmers of Saxony for the most part are well to do, but they vary greatly in their financial worth. The man whose property is worth a hundred thousand marks is not willing to become a member of a rural bank or a cooperative association of any kind with members who are worth only five thousand marks and agree to an unlimited liability tor its members. Consequently the Saxon banks are organized limiting the liability of the members in proportion to the interest they have invested in the bank. The fundamental object of the rural banks is to furnish credit to their members for working capital at the lowest rates of interest possible and not to make a profit on their business. In the

province of Saxony there are 660 rural banks. These are small village savings banks with an average membership of about 100 farmers. They are the units of the farmers' cooperative organizations of the province. At Halle there are three central cooperative organizations, with all of which the local banks stand in relation and are members: (1) The Central Cooperative Bank, which does nothing but a banking business and whose members are cooperative associations instead of individuals. (2) The Central Cooperative Association for the purchase and sale of agricultural products. This, like the central bank, has for members associations instead of persons and does a wholesale business in buying and selling agricultural products. (3) The Union of Cooperative Societies, which oversees the management of the local societies, audits their books, furnishes uniform systems of bookkeeping and looks after the organizing of new societies and does the propaganda work in promoting agricultural cooperative work in the province.

In order to become a member of the Central Bank at Halle the local association or bank must take a share in it which is 300 marks. The number of shares that the local bank or association hold is in proportion to the amount of business it does. By virtue of holding shares in the central association it is entitled to make loans from it. The farmer goes to his local bank, of which he is a member and to whom he is known, and makes his application for a loan. The bank in turn applies to the Central Association with which it has credit and secures the money and it costs the farmer ½ per cent. more interest than the local society pays the Central in order to cover the local costs of the society. The average interest rate charged by the Central Bank in 1909 was 3.92 per cent., in 1910 it was 4.34 per cent. and in 1911 was 4.39 per cent. The rate of interest paid for deposits is 3 to 3½ per cent., depending upon the current interest rate.

Credit is the first requisite of successful cooperation. When a country has a well-established system of agricultural credits it is almost certain to be thoroughly organized on a cooperative basis in other lines. This is the case in the province of Saxony, particularly in the purchase of agricultural supplies, such as fertilizers, feeding stuffs, coal, seeds and agricultural machinery.

The local banks serve the farmers both as the societies through which the purchases are made and furnish the credit for making the purchases. In this way there is a saving in the cost of doing the business and the bank knows how the money is spent.

Moral Effect of Cooperation

The development of the cooperative credit systems among the farmers of Europe has had an important influence on their social life. Aside from the independence gained in their business affairs by being

freed from the money-lenders which for the most part were usurers, they have been united in a community of interest that has widened their circle of acquaintance, given them a sympathetic interest in each other's welfare and has largely displaced the jealously so commonly existing in rural communities.

Among the peasant classes where the Raiffeissen savings and loan banks were established with unlimited liability of the members, ministers have frequently testified that they have been as important factors in the moral life of the people as the church itself. Intemperance and immorality is not permitted among the members. If a farmer takes to intemperate drinking his loan is called in by the bank. If he is neglecting the work on his farm the loan is called in. So that every farmer feels he is under the constant watch of the other members and since they are united together in a cooperative association, where if one man fails the others must pay his losses, they are all interested in each other and anxious to see every one succeed.

The application of the cooperative principle of "one for all and all for one" serves as an incentive to the individual farmer and inspires him to do his best.

Need in the United States

The farmers of the United States as yet have not appreciated the value of organizing to improve their credit. In the southern states the cotton crop must be marketed as soon as harvested to meet outstanding loans that the farmers have made at exorbitant rates of interest. The grain dealers throughout the central states know that they will be flooded with wheat and corn just before tax-paying time by farmers who are compelled to sell in order to raise money to pay taxes. Intensive systems of farming that must be adopted to adjust American agriculture to present needs means a larger working capital for the farmer, he must use more labor, more commercial fertilizers, better seed and he must drain his land. The European farmer gets twice as large a crop yield per acre as the American farmer because he spends twice as much capital in producing it. He cultivates better, fertilizes better and he takes better care of his land.

Interest rates in general are lower in the United States than they are in Germany and yet the German farmer is able to secure his credit through his cooperative organizations at two thirds the rate of interest ordinarily paid by the American farmer. In addition the loans are made on much more favorable terms and the times and methods of repayment are adjusted to suit the business of the farmers.

The advantages of the farmers organizing to sell their credit for what it is worth are not all on the part of the farmer. But for the capitalist seeking a safe investment for his money they offer a security

that can be bought at any time and is always negotiable. Such organizations serve as an economic saving between borrower and lender. The man in America at the present time who wishes to invest his money in farm mortgages must seek out such loans personally or through an agent. The punctuality with which the interest will be paid and the loan when it falls due will depend upon the personality of the farmer. But such is not the case when the loans are made through a land mortgage association and the investor instead of lending direct to the farmer buys the bonds of the association; he then knows that his interest will be paid as punctually as on a government bond; that his security has a market value and can be sold for cash any day through his bank. The establishment of the land mortgage association and selling its bonds on the open market opens up a field for investment that is now practically closed to a large class of investors.

One thing to be emphasized in regard to the success of the European systems is the fact that it has been largely due to the direct oversight that the governments have had over them. Without this government relationship they could not have commanded the confidence of the public that they have. It is hopeless to expect an equal degree of success for similar institutions in America unless they are also organized under government control, at least to the extent that the public will have absolute confidence in their solvency.

A STUDY IN JEWISH PSYCHOPATHOLOGY

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THIS is preeminently the day of preventive medicine. The campaign started many years ago to educate the people in the manner of avoiding contagious diseases has gradually been extended to other fields, until now the prophylaxis of insanity is almost as freely discussed as that of puerperal fever. And this is as it should be. Though the recovery of the already insane and the feeble-minded is seldom permanently accomplished, the outlook for the final prevention of these conditions among the potentially insane is by no means hopeless. The work undertaken by such organizations as the National Committee for Mental Hygiene, and the various allied state organizations and societies having the same general end in view are well known, and although the good already accomplished in the way of educating the people in those habits of life and thought which tend to make the development of mental afflictions less likely, is as yet inconsiderable, it promises, in the long run, to bring forth excellent results.

More and more we are coming to a realization of the importance of a good heredity. All medical men are practically agreed upon this subject. In the prevention of feeble-mindedness it is the one essential factor. It is of hardly less importance in the prevention of insanity. In an article on the hygiene of the mind, a recent writer has said "An individual who comes from normal stock, abstains from alcohol, and is free from syphilis, and escapes accidental head injury is not threatened with mental alienation."[1]

Conklin in the "Mating of the Unfit" refers to the offspring of one normal man by two separate women, one a feeble-minded girl and the other a perfectly well-balanced individual. The descendants of the feeble-minded woman were 480 in number, and of these 143 inherited the tainted mentality. The normal woman had 365 descendants and not one of them was to be classed among the mentally defective.[2]

It is also universally agreed that the propagation of tainted stock is much more likely when there is a close inbreeding of such stock. The best should be bred to the best, but different types of the same strain and close blood affinities should be avoided.

A fact so generally conceded should be applied as far as possible to the principles of marriage between individuals of both the same and

[1] A. J. Rosanoff, reprint from *New York State Hospital Bulletin*, November, 1911.

[2] Editorial, *The Lancet Clinic*, March 7, 1912.

different races. If the science of eugenics deserves any practical application at all, it should insist upon a careful study of the every-day violation of its cardinal principle by a whole race who persistently refuse to practise the very doctrine which is essential to the preservation of a sound and healthy mentality. I refer to the Jews.

In order to further elucidate the subject and to make good this rather bold assertion, I propose to prove the following propositions: First, the Jews are a highly inbred and psychopathically inclined race. Second, the prevalence of mental affections among them is almost entirely due to heredity.

That the Jews are as a matter of fact racially pure is a statement that must not be taken absolutely in a literal sense. All races have received admixtures of some outside blood, but it is undoubtedly true that, since the time of the prophet Ezra and his campaign for racial purity which was begun at the time of the return from the Babylonian captivity, 536 years before Christ, the central stem of Judah has remained practically free from admixture with other races. Any student of the old testament can easily substantiate the statement that violations of the law against marriage with the heathen races, by which they were surrounded, were, from that time on, most summarily punished. The feeling against such procedure grew in intensity until, at the time of the fight for the maintenance of Jewish independence under the Maccabees, it had reached such a degree of fervor that rabbinical decrees forbade friendly social intercourse with the Gentile on any pretext whatever. There is no doubt but that the Jews inter-married and inbred among themselves on an ever increasing scale clear up to the time of the fall of Jerusalem.

After the Dispersion, there is some difference of opinion as to the degree with which they maintained the racial purity which they had been over 600 years in establishing. The weight of evidence, however, is all in favor of the view that they did not abandon the time-honored doctrine of racial solidarity. During Roman times and the dispersion throughout the Mediterranean littoral, the rabbinical decrees were still vindictive in their treatment of the subject. To such extreme lengths did they go, that the *Goy* or Gentile party to the contract was regarded as having no right at all, but was considered like the slave, as having a status that rendered him incapable of connubium with the Jews.[8]

Those anthropologists who cite the fact that there were a great many converts to Judaism immediately after the fall of Jerusalem, and that the Jews thus received a great deal of Roman blood into their veins, overlook the fact that these converts were the very ones from whom the Christians in turn drew the majority of their converts. Thus the Judaized Romans were almost immediately lost to Judaism.

[8] Ephraim Feldman, "Intermarriage, Historically Considered," p. 19.

Likewise the alleged conversion of the Chazars, a Tartar tribe of Russia, was in reality confined to the ruling classes and their immediate court dependents, the main body of the people remaining free from the admixture with the Jewish proselytes.[4]

During the middle ages the Christians themselves put a ban on intermarriage and thus the rule against its practise was doubly enforced.

Since the time of Napoleon and the consequent removal of the political disabilities of the Jews, there has been no increasing tendency to take outsiders into the Jewish fold. Even in New York, where the social and business relations between Jew and Gentile are perhaps closer than in any other city in the world, there is practically no tendency to encourage mixed marriages. Although it is manifestly impossible to obtain exact figures upon this subject, all the obtainable data go to support the view that the inbreeding is going on here as much as in any other place in the world. M. Victor Safford, who has investigated extensively the results of immigration upon the ethnic composition of our population, has said that a study of the marriage certificates in New York City, while not giving sufficient grounds to absolutely prove the contention that intermarriage between Jew and Gentile is rare, does, nevertheless, justify the *belief* that such is the case.[5] The children resulting from such marriages almost invariably marry either Jews or persons who, like themselves, are only half Jews. Thus, what little departure there is from the principle against mixed marriages has no tendency to introduce fresh blood into the central stem. Hirsch has said that if all the Jews had remained Jews since the time of Christ that there would be 100,000,000 in the world to-day.[6] If we grant the truth of this rough approximation, it only serves to show that the result of all this "marrying out" has been an ever-increasing practise of inbreeding in the pure central stock. When we consider that the total number of the race is small, being probably not much over 12,000,000 altogether, we can easily see that there is not enough variety of blood among the members of the different Jewish families to avoid frequent consanguineous marriages.

Theoretically, the Jews are compelled to observe marriage customs which result in racial incest. Practically, it is well known that they really do marry their own cousins much more than do the people of other races. Jacobs says that they are probably three times as guilty in this respect as others.[7] I think we must concede the point that they are a highly inbred and closely related race. This fact undoubtedly accounts for the very strong racial characteristics which they possess.

[4] W. D. Morrison, "The Jews under Roman Rule," p. 413.
[5] M. Victor Safford, private communication, June 29, 1912.
[6] William Hirsch, "Religion and Civilization," p. 579.
[7] Joseph Jacobs, "Studies in Jewish Statistics," Appendix, p. 4.

I am not here concerned with any of those characteristics except the psychopathic ones. If they are endowed with exceptionally great mental gifts, it is beyond the scope of this paper to consider them. What I now propose to show is that they suffer from constitutional mental inferiorities, or psychopathic tendencies to a degree entirely out of proportion to the occurrence of such infirmities among the general population. First we will consider our own country. As fully two thirds of the Jews in the United States are in New York, it will be unnecessary to go out of that state to procure the evidence. In the year ending September 30, 1909, out of a total of 5,222 admissions to all the New York State hospitals for the insane, 488 were Jews.[8] While these statistics do not show the total number of Jews insane from all causes to be greatly in excess of the ratio which they bear to the general population, they plainly indicate that they do not fall below the general average, and when we come to analyze them in detail we find that they show a disproportionate number of cases due to constitutional mental inferiority. Taking those insanity groups which in all classifications are universally admitted to be due to bad heredity, the total number among the non-Jews was 2,297 or 48 per cent. of the total number of non-Jewish admissions. On the other hand, 65 per cent. of the Jewish insane belonged to the constitutionally inferior groups.[9]

TABLE OF ADMISSIONS TO THE MANHATTAN STATE HOSPITAL FOR THE INSANE CLASSIFIED BY RACE AND NATURE OF PSYCHOSIS FOR THE YEAR ENDING SEPTEMBER 30, 1908[11]

Psychoses	Irish	Jewish	German	U. S.	Italian	Negro
Senile psychoses	9.80	2.87	6.70	7.14	3.70	9.80
General paresis	7.59	14.05	20.10	17.46	9.87	29.41
Alcoholic psychoses	27.69	.32	11.85	11.90	8.64	7.82
(Dementia præcox)	13.48	27.47	14.95	16.66	23.44	13.72
(Manic-depressive insanity)	16.66	28.43	12.89	18.25	13.58	9.80
Epileptic psychoses	2.20	1.59	4.64	3.17	4.93	3.92
Other psychoses	22.58	25.27	28.87	25.42	35.84	25.53
Total number of each race	408	313	194	126	81	51

The practical freedom of this race from the alcoholic psychoses is a matter of common knowledge. Now alcohol was responsible for over ten per cent. of the insanities admitted to the New York state hospitals for the year ending September 30, 1909. But the Jews admitted for alcoholic psychoses for the same period constituted less

[8] Twenty-first Annual Report of the State Commission in Lunacy, Statistics of the Insane, tables 4 and 14.

[9] These percentages are deduced from tables 4 and 14, Statistics of the Insane, State of New York, 1908–09.

[11] De Fursac and Rosanoff, quoting Dr. Kirby in "Manual of Psychiatry," p. 6.

than one per cent. of the total Jewish admissions. Notwithstanding the fact that the Jews are thus almost entirely uninfluenced by the greatest of the acquired or accidental causes of insanity, their total number of insane does not fall to the level of the average for the general population. Thus out of 1,762 admissions to the Manhattan State Hospital for 1910–11, there were 455 Jews; that is to say, they made 25.9 per cent. of the total admissions. This is nine tenths of one per cent. more than their usually estimated relation to the general population of the community from which they were recruited.

Reliable data from foreign countries serves to show that, notwithstanding his freedom from alcohol, the Jew still contributes more than his share to the general insane population. Thus in Germany for the period 1890–1902 there were to the 100,000 of population, an annual average number of 67 insane and feeble-minded Jews as against 49 of the non-Jewish population. The congenital idiocies and congenital imbecilities showed an especial disproportion against the Jews, they having 4.51 as compared to 2.75 among the non-Jews.[10] That the proportion of the constitutionally inferior is especially large is shown by a reference to the subjoined table, which is taken from De Fursac's and Rosanoff's latest work on psychiatry. It will be noted that notwithstanding the fact that they have practically none of the psychoses which are due to alcohol, the Jews come second in point of number of admissions.

In this connection the percentage of the Irish admitted for alco-

TABLE OF MENTAL DEFECTIVES AMONG IMMIGRANTS (IDIOTS, IMBECILES, FEEBLE-MINDED). ANNUAL REPORT COMMISSIONER GENERAL OF IMMIGRATION, 1911. REJECTED FOR THE YEAR 1911

	Total	Mental Defectives	Per 100,000
Finnish	9.779	0	0
Russian	18,721	0	0
Spanish	8,068	0	0
Magyar	19,966	1	5
Greek	37,021	2	6
Dutch and Flemish	13,862	1	8
Scandinavian	45,859	4	9
Bohemian (Czech)	9,223	1	11
Ruthenian	17,724	2	11
Pole	71,446	9	12
Slovak	21,415	3	15
Italian (North)	30,312	5	16
Croatian, Slovenian	982	2	22
Italian (South)	159,638	36	23
English	57,258	13	23
German	66,471	15	23
Scotch	25,625	6	23
Irish	40,246	11	27
Hebrew	91,223	26	28
French	18,132	6	32

[10] Maurice Fishberg, "The Jews: A Study of Race and Environment."

holic psychoses exactly equals that of the Jews admitted for dementia præcox. Now, dementia præcox is classed as one of the insanities depending upon a constitutionally inferior mental make-up, as is likewise manic-depressive insanity. These two constitutional inferior groups which are universally agreed to rest upon a bad heredity, alone account for over 55 per cent. of the insanities among the Jews in the above table. Statistics could be multiplied almost indefinitely to show similar results.

Among the frankly feeble-minded, the Jews stand next to the top of the list of those immigrants who are deported on this account. The report of the Commissioner General of Immigration for 1911 shows that the French are the only ones who surpass them. In this connection it is well to note that over one half of the French immigrants for the year 1911 was recruited from the ranks of the French Canadians, who are a notoriously inbred and defective stock.

If it be objected that the foregoing table represents one year alone and can not be properly used to aid in drawing such wholesale conclusions, the answer is two-fold. In the first place this was a year in which the general average of Hebrew defectives was proportionately smaller than in other years. For instance, in 1907 nearly one third of those certified at Ellis Island as mentally defective were of this race, although they did not average over 14 per cent. of the total number of arrivals.

In the second place the number of feeble-minded children in the public schools of New York City is disproportionately large among the Jews. Thus of 317 mentally defective children selected at random from ungraded classes, Miss Anna Moore in 1911 found that there were 130 Hebrews, 40 Italians, 35 Germans, 20 Irish and 9 negroes.[12]

An attempt has been made to deny the ethnic or racial relation with the greater prevalence of feeble-mindedness and insanity, which the foregoing data would naturally seem to indicate. Thus it has been said that the birth-rate among the Jews being lower than that of the general population, there is consequently a larger proportion of adults among this race as compared with others, and insanity being chiefly a disease of adults, it follows that its greater prevalence among the Jews is apparent rather than real.

To explain the large number of feeble-minded the argument runs in this wise: Although the birth-rate among the Jews is low, Jewish parents take better care of their children than others, consequently more survive those illnesses which result in mental deterioration.

The chief fallacy in this argument lies in the fact that those who use it neglect to state that feeble-mindedness is overwhelmingly a dis-

[12] Miss Anna Moore, Report published by State Charities Aid Association, 1911.

ease of childhood, death for one cause or another intervening in the majority of instances long before the age of the natural expectation of life is reached. When we consider this universally admitted fact, it becomes apparent that at least one of the aforesaid explanations must be wrong. For, if children of Jewish parents survive in such disproportionately large numbers as to account for the seeming excess of feeble-mindedness, it naturally follows that this survival must offset the diminished birth-rate and serve to maintain the normal relation between child and adult population.

If the difference in the relation of adult to child population really exists in a sufficient degree to be a factor at all in the explanation of the degree of prevalence of insanity and feeble-mindedness, the logical argument would be as follows:

1. The proportion of Jewish adults to the general population is greater than among others, consequently the proportion of the child population is less.

2. Feeble-mindedness is a disease of childhood.

3. Conclusion: Being fewer Jewish children there are fewer feeble-minded among Jews than among others.

If we reverse the argument and assume the premise that more Jewish children survive than among others we should have the following syllogism:

1. The proportion of Jewish children to the general population is greater than among others, consequently the proportion of the adult population is less.

2. Insanity is a disease of adults.

3. Conclusion: Being fewer Jewish adults there are fewer Jewish insane.

A consideration of the foregoing examples of *reductio ad absurdum* only serves to confirm the belief that, after all, there must be some intimate relation existing between the racial, or inherent ethnic characteristics of the Jews and the greater prevalence of insanity and feeble-mindedness among them. In no instances shall we find any reliable data that show the proportion of feeble-minded and insane among the Jews to be less than among the general population; in most countries it is undeniably larger, and in every instance the number of Jews suffering from mental defects are recruited from the ranks of the congenitally inferior in a far greater proportion than is the case among the non-Jews.

In the light of our knowledge of the laws of heredity, there can be but one thing responsible for the above-described condition. It must necessarily have been brought about by too close inbreeding.

That the excessive number of constitutional inferior insanities has a partial explanation in the fact that long centuries of inbreeding have

produced a race with a paranoid make-up seems not altogether improbable. The general paranoid attitude of the race is shown in an almost universal tendency to assume the possession of superior racial mental qualifications, and when these are denied or in any way gainsaid, to fail to appreciate the point of view of the one who opposes them.

This idea of superiority to other people is so inbred that it has probably become a hereditary character for which the individual is entirely irresponsible. But a paranoid make-up is not particularly dangerous to its possessor who is otherwise normal, unless by great stress or a very unusual combination of disagreeable experiences this tendency be diverted into abnormal channels.

The chief danger lies in the accentuation of the character by too close inbreeding with those having a like tendency. In fact, the general attitude of the person who has this paranoid make-up in a mild degree may be said to be an enviable one rather than otherwise. He is aggressive in upholding his rights, suspicious of attempts to thwart him in the pursuit of the same, and strives constantly to reach the goal of his ambitions. These are all admirable traits. It is only when they become accentuated to the point where they are pervaded by delusions of grandeur and persecution, that they render the person possessing them a menace to society.

To return to the thought expressed at the beginning of this paper, that the prevention of mental diseases is quite the most important part of their treatment, it would seem that the Jews have it in their power to ultimately stamp out the feeble-minded and insane from among their race. The way in which they can do this must be plain to whoever has followed the gist of my argument. It is all a question of eugenics. A little more care in the matter of consanguineous marriages and a quick and thorough departure from the old beaten tracks which forbid the introduction of non-Jewish blood into their veins, will, in the course of a few generations, redeem them from the unhappy mental state into which they have fallen.

THE LANGUAGE OF METEOROLOGY

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IN discussing the vocabulary of any branch of science one is embarrassed by the fact that scientific language in general is a neglected subject. The principles of scientific terminology and nomenclature (on the etymological side) are not, to my knowledge, taught in modern curricula; their formal exposition belongs to the scholarly literature of a past generation; and the writings of our contemporaries bear evidence of the fact that philology does not now enter to so large an extent as formerly into the equipment of the average man of science.

The student of to-day is, as a rule, left to make his own generalizations on this subject from the transformations in the technical vocabulary that happens to come under his observation; and his inductions suffer in proportion as these transformations become less orderly. When he arrives at the creative stage, and is called upon to label his contributions to knowledge, he is apt to still further increase the disorder of the language; and thus an interaction is going on that would speedily lead to chaos, if it were not checked by powerful though unrecognized laws governing the development of human speech—a pervasive "Sprachgefühl" that saves the language from falling into rapid ruin, though it can not protect it from gradual deterioration.

The fact that the underlying principles of terminology and nomenclature are not, to say the least, clearly formulated in the minds of most men of science makes it desirable, in discussing a particular group of technical terms or names, to begin far back of one's subject—just as it is desirable for a newspaper writer on Halley's comet to begin by enlightening the public in regard to the heavenly bodies in general. However, it is not practicable to follow such a plan within the limits of a brief paper. In the present case I shall cut the Gordian knot by simply referring my readers to the two statements of fundamental principles that I have myself found most illuminating—viz., the fourth book of William Whewell's "Novum Organon Renovatum" and Dr. Lereboullet's article "Etymologie" in the "Dictionnaire encyclopédique des sciences médicales"—and proceed at once to a discussion of some salient features of the language of meteorology.

One curious fact about this language is that a considerable part of it is unknown to meteorologists. Hundreds of useful terms have been introduced to fill the gaps in its vocabulary—some highly felicitous, others at least tolerable—only to sink into speedy oblivion, leaving their places unfilled. Take, for example, the names of the isograms—and

the name "isogram" itself. The latter, denoting a line that represents equality of some physical condition on a map or diagram (the isotherm and the isobar being the most familiar examples, is a convenient generic term, the need of which must have been often felt long before it was invented, in the year 1889, by Francis Galton. Yet to this day it is unknown to most meteorological writers, who continue to use an awkward periphrasis to express this every-day idea.

Several meteorologists have drawn lines connecting places of equal evaporation; very few have ventured to give these lines a name. There is no inconvenience in referring once or twice in a scientific memoir to a "line of equal evaporation." Suppose, however, one needs to mention the same thing fifty times. One is almost driven to the necessity of substituting a single word for this long phrase; and thus certain writers have, in fact, coined the terms "isoatmic line" and "isothyme"; but neither of these has gained currency in the habitual vocabulary of meteorologists.

In all, some eighty meteorological isograms have been named; but of their names less than a score are generally familiar, and many are almost completely forgotten.

During the last two or three years the recognition of the importance of the "barometric tendency" in weather forecasting has made us tolerably familiar with the "isallobar"; but what of the "isallotherm"? Lines of equal temperature-change have been drawn on forecast charts for a great many years. Their name, however, has just been invented, and is hardly yet known to the practical forecaster.

There is a marked reluctance on the part of contemporary men of science to contribute to the scientific vocabulary. This is perhaps due to the growing ignorance of the principles of etymology to which I have already referred; though it may be also the token of a reaction from the pedantry of an older generation, which cumbered the language with terms too labored for daily use, and often with names of things that might well have been left nameless.

I have in mind a number of lexical curiosities that furnish diversion to any one who chances to read a memoir by A. Piche, "La Météorologie dans le Département des Basses-Pyrénées." From this work we learn that "meteorologistotheory" is the branch of science dealing with meteorologists; that "meteorologistopiry" has to do with experiments in the training and organizing of meteorologists; that "meteorologistonomy" relates to meteorological administration; that "meteorologistotechny" is the art of applying the laws relating to the production of meteorologists, their arrangement into groups, and the development of their labors; that "meteorologistosophy" is the philosophical study of meteorologists; etc. In short, M. Piche has stuck pins through his meteorologists as if they were so many butterflies, and has made them the subject of a new branch of natural history. His

terminology is so terrifying that we are thankful the meteorologists had individual names before he got hold of them; otherwise we shudder to think what he might have done in the way of nomenclature! The same ingenious Frenchman invented an instrument for measuring the sensible temperature which he called at first the "calorisoustractometer"; but later he took pity on humanity and changed its name to "deperditometer."

Of the two evils—a clumsy term or none at all—the former is certainly to be preferred. There can be no progress in ideas without a corresponding progress in language. This fact is emphasized by Whewell; and he cites in illustration the cases of Cæsalpinus in botany, and Willughby in ichthyology, each of whom introduced excellent systems of classification which failed to take root or produce any lasting effect among naturalists because they were not accompanied by corresponding nomenclatures. No one recognized this truth more clearly than Linnæus, whose great contributions to botany were surpassed by his contributions to the *language* of botany. Whewell quotes a maxim from Linnæus's "Botanical Philosophy,"

Nomina si nescis perit et cognitio rerum,

which ought to be taken to heart by the many scientific men of to-day who are conspicuously shirking their obligations to the technical vocabulary.

In the history of meteorology there are innumerable instances of important ideas that led a precarious existence for years, almost ignored by meteorologists at large, because no one had crystallized them by giving them names. Think of the number of conceptions that owe their present definiteness in our minds to the felicitous terminizing of Ralph Abercromby! The seven typical forms of isobars are familiar examples. Another is the generalization "recurrence," under which term Abercromby united the many cases of the supposed tendency of particular types of unseasonable weather to occur from year to year at about the same period—Indian summer, the "Ice Saints," the "Lammas floods," the "January thaw," the "borrowing days," and a number of other similar interruptions in the regular march of the seasons—all of them more or less elusive when submitted to a rigorous analysis, but none the less deeply-rooted conceptions in the popular mind. Individually these supposed occurrences are familiar to all meteorologists, but we should probably sometimes lose sight of their generic similarity had not Abercromby given them a handy generic name.

Probably in no branch of science is the vocabulary more confused than in atmospheric optics; especially in English. This particular subject affords so many examples of the vices of the existing language of meteorology that we may profitably consider it at some length.

In a publication which, I regret to say, bears the official imprimatur of the Weather Bureau,[1] I find a definition of the "solar aureole, corona, or glory." These names are stated to belong to the familiar phenomenon of diffraction rings around the sun; and the question arises—Why three names for one thing? Etymologically one is as good as another; but the single term "corona" was long ago appropriated to the phenomenon in question. If we consult Pertner's "Meteorologische Optik," we shall find that, according to this authority at least, the aureole is *not* identical with the corona. A separate name was desired for that inner portion of the complete corona which is, as a rule, the only part visible; extending from the blue-white zone around the luminary to the reddish brown circle adjacent, but not including either indigo or violet. Pernter was, I believe, the first person to distinguish this part of the corona under the name "aureole." The glory, again, is something quite different. This is not seen around a heavenly body, but surrounds the shadow of the observer's head—strictly speaking, of the observer's *eye*—cast upon a cloud or fog-bank. In the phenomenon of the Brocken specter the glory constitutes the "Brocken bow"—though the specter and the bow are persistently confused in the dictionaries and in the literature of meteorology.

This leads us to a further hopelessly confused statement in connection with the definition above quoted, reading as follows: "A smaller circle surrounding the shadow of the observer's head is called an anthelion, aureole, glory, or fog shadow." The word "anthelion" has, indeed, been used persistently in this sense in English literature; though such a use has never been countenanced in French or German. Bravais and his successors applied the name "anthelion" to what is sometimes called in English the "countersun"; viz., a white image of the sun seen at the same altitude as that luminary, but opposite it in azimuth—one of the rarer phenomena of the great halo family. Although this, the preferable, use of the name is absolutely ignored in the English dictionaries—which uniformly confuse the anthelion with the glory—it is not quite unknown to English writers. I find the "anthelion," in this sense of the term (as observed in the year 1762), described and figured in the "Philosophical Transactions" (abridged), Vol. 11, p. 532. A similar use of the term occurs in Howard's "Climate of London," 2d ed. (1833), Vol. 1, p. 222. As to "aureole," we have already seen how Pernter has desynonymized this term. "Fog shadow" is obviously a most inappropriate name for a ring of *light*. In short, the sentence above quoted, revised in accordance with the requirements of accurate terminology, would read: "A smaller circle surrounding the shadow of the observer's head is called a *glory*." The three other names are untenable.

[1] *Monthly Weather Review*, Vol. 33, p. 527. This is, however, substantially a quotation from the Smithsonian Meteorological Tables.

Although I have quoted a Weather Bureau publication—because it happened to lie nearest at hand—the example selected is a fair specimen of the loose language of a majority of writers on atmospheric optics. In fact, the vocabulary is so confused that one can hardly write of any but the commonest of the photometeors without defining each term he uses; and I am not sure that even the names of the commonest are wholly unequivocal. In a recent number of *Nature*—a journal which is usually a purist in scientific English—the beautiful circumzenithal arc, Mascart's "upper quasi-tangent arc of the halo of 46 degrees," was referred to as a "zenith rainbow." Still more startling is it to find the new edition of Wood's "Physical Optics" ignoring the term "corona" altogether in describing the diffraction rings around the sun and moon.

In contrast to the prevailing confusion in the English vocabulary of this subject, we find that the labors of Pernter have led to the adoption of a nearly uniform terminology in recent German literature; but this writer shares with his compatriots a prejudice in favor of native terms that detracts much from the value of his contributions to the universal language of science. Thus, while he has adopted the Greek word "halo," he prefers to call a corona a "Kranz," and he clings to "Hof" as a general name for the heliocentric circles of all kinds. In fact, very few Greek or Latin names appear anywhere in his great treatise on atmospheric optics. Of course, this fact is merely typical of the almost universal preference of German science for linguistic isolation; a subject too large to enter upon here.

In French, the complicated terminology of halos was set in order by Auguste Bravais, and his labors have been admirably seconded in our own time by Louis Besson. Fortunately French science still prefers a Græco-Latin vocabulary, and the terms it introduces are easily taken over into English. No adequate account of halos has yet appeared in our language. Whoever undertakes to write one will hardly err in adopting the Bravais-Besson terminology *en bloc,* with only the necessary idiomatic modifications and without regard to the practise of earlier English writers on the same subject.

In the brief space remaining at my disposal I think I can not do better than to refer specifically to a few meteorological terms, of more or less recent origin, that deserve a wider use in scientific literature than they now enjoy.

Beginning at the top of the alphabet, I find that the branch of meteorology dealing with upper-air research is not yet known to all meteorologists as "aerology." This term, proposed by Köppen, and officially adopted at the Milan meeting of the International Commission for Scientific Aeronautics in 1906, is so well adapted to fill a serious gap in our vocabulary that one is surprised at the slow progress it has made in English. This is all the more surprising because, in

spite of its Greek etymology, it was promptly accepted by the Germans, and is now fully established in their language. The expression "scientific aeronautics," still incorporated in the name of the international commission that has the oversight of aerological matters, is an obvious misnomer as applied to the exploration of the free atmosphere, notwithstanding the fact that aeronautical methods and appliances are largely used in this field of research.

The most remarkable occurrence in the history of aerology was the discovery, in 1902, of a region of the atmosphere originally called by its discoverer the "isothermal layer"; a name that he has since abandoned in favor of "stratosphere." A number of other names have been proposed as alternatives—in some cases for reasons that, to any one familiar with the natural history of scientific terms, seem decidedly frivolous. Thus, some of our English confrères objected to the original name because there was no certainty that the so-called "layer" had an upper boundary—an objection that has perhaps been disposed of recently by Dr. Alfred Wegener. Mr. Dines, one of the ablest of aerologists, prefers to speak only of "isothermal columns" in the atmosphere; but this plan leaves the important stratum as a whole without a name. There is every indication at present that Teisserenc de Bort's second term, "stratosphere," will ultimately prevail. It commends itself by its consonance with the term "troposphere," applied by the same investigator to the region of clouds and convective disturbances, and with Wegener's recent tentative names for supposed higher strata of the atmosphere—"hydrogensphere" and "geocoroniumsphere"; and all of these conform to the well-established terminology of "atmosphere," "hydrosphere" and "lithosphere."

Meteorology has recently profited, as to terminology and otherwise, by the writings of Henryk Arctowski, who, though a Pole by birth and a Belgian by adoption, wields a very facile pen in English. M. Arctowski is responsible for the convenient words "pleion" and "antipleion," denoting, respectively, regions of positive and negative departure from a normal. Thus, a temperature pleion, or "thermopleion,"[2] lay over western Europe during most of the summer and early autumn of 1911. Lines of equal positive and negative departure from normal temperature (not "anomalies," which are departures of local means from the means of latitude circles) were unnamed until Arctowski called them, respectively, "hypertherms" and "hypotherms." All these terms are correctly formed from Greek roots, are easily assimilable into our language, and are well fitted to give definiteness to a group of ideas that formerly suffered in this respect by the lack of a terminology.

[2] M. Arctowski's terminology is not quite consistent, since he does not speak of "thermoantipleions," but of "thermomeions." As "antipleion" is an awkward form in combinations, it is unfortunate that it was adopted as the generic term. "Meion" is preferable.

Nevertheless, their use has not spread since they were proposed, two or three years ago. It is to be hoped that they are not destined to share the oblivion of some analogous terms relating to atmospheric pressure proposed about forty years ago by Prestel; viz., "pleiobar," "mesobar" and "meiobar."

Purely English terminology has received some useful amendments at the hands of Dr. Hugh Robert Mill, who in this respect is carrying on the worthy traditions of "British Rainfall." Thus he has balanced Symons's terminology of droughts—the "absolute" and the "partial" drought—by introducing the term "rain spell" for a period of more than 14 successive days with rain. This expression, however, like the term "rain day," is one that would need to be redefined in other countries. Dr. Mill has rendered an even more useful service to precise terminology by distinguishing between the words "mean," "average" and "general." He speaks, for example, of the mean temperature at Camden Square during the month of June, 1900; the average temperature at the same place in June during a ten-year period; the general rainfall over the whole county of London in May, 1910, and the average general rainfall over the same region for a term of years.

British meteorologists have also succeeded in establishing a working terminology in English for the various deposits of frozen moisture that have occasioned so much fruitless discussion at international meteorological meetings. The Meteorological Office now applies the term "rime" to the rough deposits due to fog, and "glazed frost" to the transparent smooth coating usually caused by rain which freezes as it reaches terrestrial objects. The ambiguous expression "silver thaw" has been discarded in British meteorology.

The endless subject of cloud terminology and nomenclature can not be discussed in this paper; but I wish to call attention to one term in this connection recently introduced by M. Besson. This is the name "nephometer" for an instrument used in measuring the amount of cloudiness, as distinguished from the familiar "nephoscope," by which we observe the positions and movements of individual clouds.

German meteorologists have lately introduced the all-Greek names "chionometer" and "chionograph," and the hybrid "nivometer," for the instruments used in measuring snow. Although these terms will hardly displace "snow-gage" in English, we shall probably find it convenient to use their derivatives; *e. g.*, "nivometric"; just as we use "pluviometric," though we generally avoid "pluviometer."

The name "ceraunograph" applied by Odenbach in 1891 to his variety of the thunderstorm-recorder now seems destined to become the generic and international designation for the numerous instruments of this class. Particular forms have been known as "thunderstorm-recorders," "lightning-recorders," "brontometers," "brontographs," "ceraunometers," "electroradiographs," etc. "Ceraunophone" will.

accordingly, be the natural designation of the modification of the ceraunograph in which a telephone-receiver takes the place of a recording pen.

Our Weather Bureau has recently contributed to the meteorological vocabulary the name "kiosk," applied to a little pavilion in which working meteorological instruments are displayed for the benefit of the public. Although the connotations of this word are hardly consistent with the style of architecture adopted for these structures in America, no better designation has been proposed, and it is safe to assume that "kiosk," as well as the object so named, has come to stay. It is rather curious that, although "Wettersäulen" have been familiar objects in Germany for half a century, their use has only recently spread to English-speaking countries, and the need of an English name for them has only recently made itself felt.

When the first complete English meteorological dictionary makes its appearance it will need to take account of fully ten thousand words and phrases; and in connection with hundreds of these much work must be done in tracing their vicissitudes and in bringing them into something like conformity with a systematic and workable language. The terms I have mentioned in the foregoing paragraphs are, in the language of the day, "a drop in the bucket."

In closing, I wish to repeat a recommendation that I recently made to the International Meteorological Committee, through the kind intermediation of the chief of the Weather Bureau, in behalf of the creation of an international commission on terminology, analogous to the commissions already established by the committee on various other meteorological subjects. The utility of such a step is well attested in the history of other sciences. In electricity, for example, the useful names of the electrical units—"ohm," "volt," "ampere," "coulomb," "farad," "joule," "watt" and "henry"—were all promulgated by formal international agreement.

The International Meteorological Committee and Conferences have, it is true, given us official definitions of a few terms; but such work can not be done on an extensive scale save by a body especially created for the purpose and having far more time at its disposal than is available at the ordinary triennial assemblies of meteorologists.

Pending the consummation of this wish, let me urge meteorologists to familiarize themselves with the neglected language of their science; to avoid coining needless synonyms of terms that already exist; and, when a new term is really needed, to create one with due regard to the analogies of the language and its availability for international use. Generally speaking, only Greek and Latin derivatives answer the latter requirement. If a meteorologist feels himself unequal to framing a valid word from the classical vocabularies, he can always appeal for aid to some friendly colleague of philological attainments.

THE SWEDEN VALLEY ICE MINE AND ITS EXPLANATION

By MARLIN O. ANDREWS

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THE Sweden Valley Ice Mine, one of the unexplained mysteries of nature, is located about four miles east of Coudersport, the county seat of Potter County, Pa. A similar phenomenon is situated on Dingman Run, about three miles west of Coudersport. These are natural ice-manufacturing plants, running under full head during the warm season of the year, but shutting down entirely during the cold months of winter, when there is plenty of ice and snow to be had elsewhere and when it would seem to be the most natural time for the formation of ice at these places.

To learn something of the history of the Sweden Valley Ice Mine we must go back to the time when the Indians were the chief inhabitants of this particular section of the country.

A certain tribe knew the location of deposits of silver and lead, which they carefully guarded against discovery both by other bands of Indians and by the few white settlers in that vicinity. As the whites became more numerous the Indians were driven farther west, taking their mineral secrets with them, as well as the scalp of one white hunter who accidentally discovered one of their lead mines. For years accounts of these mines were handed down from one generation to another, until, having become partially civilized, the Indians returned to recover, if possible, some of their lost wealth. They came in bands of five or six and searched the country thoroughly in the vicinity of Coudersport and Sweden Valley, but without success. The country had been so changed by the advance of civilization that they were unable to follow the directions given them by their ancestors and were finally obliged to abandon the undertaking.

These strange, unexplained actions on the part of the Indians naturally aroused considerable curiosity among the residents. They surmised that the Indians were searching for minerals, and the ground was again thoroughly gone over, but with no better success.

A year or so later, in 1894 or 1895, a Cataraugus Indian came to Coudersport, got a lunch and walked off into the woods. After some time he returned with some fine specimens of silver ore which he exhibited to the amazed loungers who gathered around him. He then disappeared without telling any one where he was from, where he secured the ore or where he was going.

The result of this visit was only natural. Silver mining was the

topic of conversation whenever two or more persons got together. Another search was organized which resulted in the discovery of the Sweden Valley Ice Mine.

Mr. John Dodd and Mr. William O'Neil were prospecting near Sweden Valley when, underneath four or five inches of moss, they found a thin layer of solid ice. After leveling off a space about fifteen or twenty feet square they dug a shaft about six feet square by twelve feet deep. At a depth of nine feet they found petrified wood, impressions of leaves, ferns and other vegetation, also bones which were pronounced to be human. At a lower depth a peculiar kind of rock was found which they thought might contain gold or silver. Some of this

SHOWING THE OPENING AT THE TOP OF THE SHAFT.

was assayed and found to be of no value. At a depth of twelve feet an aperture was found from which came a cold draught. This was thought peculiar, but nothing was done to investigate farther and the work was abandoned.

The following spring Mr. Dodd found a considerable amount of ice in the mine, but thought that it had gathered there during the winter and had not yet melted. However, as the warm weather advanced, the quantity of ice, instead of melting as was expected, began to increase, and by the middle of July the sides of the shaft were covered with a coating of ice a foot or more thick and large icicles were forming from the opening at the top.

As winter again came on, the ice began to disappear until the cave

LOOKING DOWN THE SHAFT, SHOWING THE ICE-COVERED STEPS.

was nearly free from the summer's product. This phenomenon has regularly been repeated each year since its discovery.

Mr. Dodd, who owns the land, had a small building erected around the mine, leaving the roof, directly over the shaft, open so as to allow the rays of the sun to beat in upon the ice formation. The beautiful woods surrounding this spot make an ideal place for picnics and it has become a favorite place for visitors to spend an afternoon, and incidentally cool off.

Two years ago (1910) the bottom of the shaft settled eighteen inches, leading to an experiment by Mr. Dodd. He says that two sticks of dynamite were placed about eight feet back into a crevice at the bottom of the shaft and fired without turning a stone or dislodging any earth in the shaft. A possible conclusion is that there is a cave underneath the mine large enough to absorb the shock of the explosion. Nothing more has been done in the way of investigation.

The Dingman Run Ice Mine is a more recent discovery, being found on June 15, 1905, on Dingman Run on the farm of Mr. Pelchy. Mr. Pelchy, with the help of another man, was clearing up some brushland for farming when, in order to get a better foothold on the steep hillside, he tore away a little of the moss, which was several inches deep at that place, and found pieces of ice.

Having heard of the ice mine at Sweden Valley he began to dig in the hope of discovering a similar phenomenon on his own farm. He made an opening in the hillside ten feet deep by twenty across,

finding crevices in the rock from which he took chunks of ice weighing twenty and twenty-five pounds. Nothing more was done to bring this mine to the notice of the public and consequently it is known to but very few people even in Coudersport.

Although the Sweden Valley Ice Mine was discovered in 1898, it is practically unknown to-day. It is astonishing how many people within a few miles have never visited it nor heard of it. Recent inquiry (March, 1912) at the United States Geological Survey, Washington, brought forth the following response:

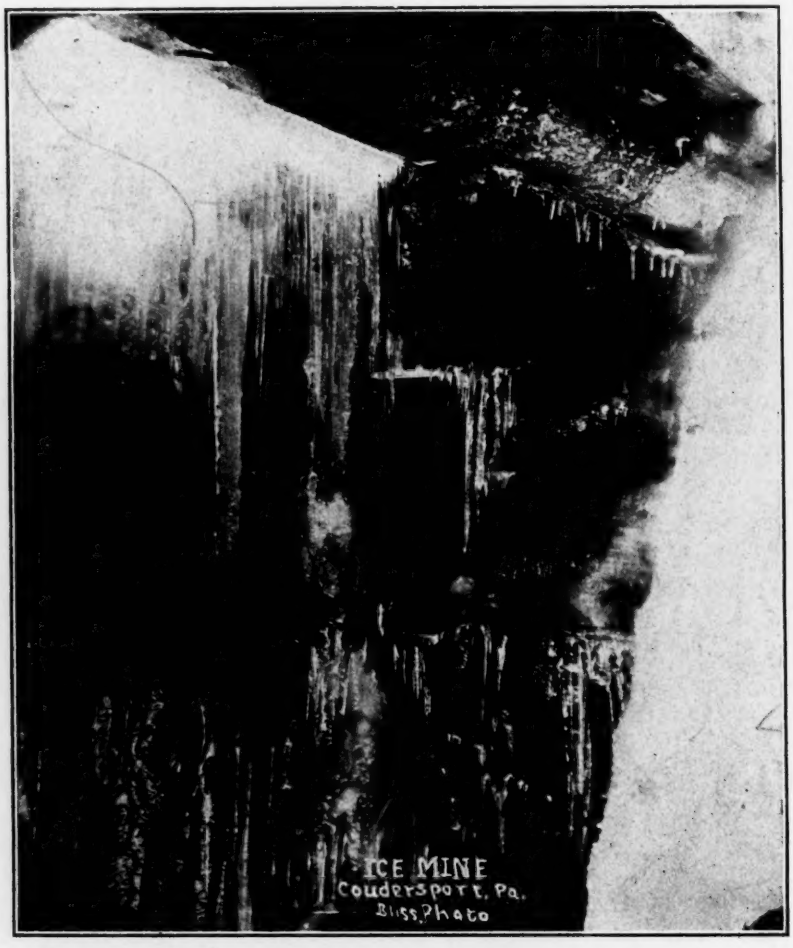
> There are in northern Pennsylvania, on the high plateau, several localities where, during the winter, snow and ice accumulate in large quantities under the protection of cliffs and caves, so that ice is obtainable from these sources during the succeeding warm season, but the Geological Survey has no knowledge of any ice mine in which ice is actually forming during the warm season.

The reason the U. S. Geological Survey has no record of these phenomena is that their survey in Potter County has never been completed and no atlas of the county has ever been published.

Further inquiry brought the following reply:

ICICLES FORMING FROM THE TOP OF THE SHAFT.

We find that phenomena similar to that described by you are not unknown and have been discussed in numerous papers. One of the best of these is the article on the Decorah Ice Cave and its explanation by Mr. Alois F. Kovarik, *Scientific American Supplement*, November 26, 1898, pp. 19158 and 19159. Dr. Samuel Calvin in his geology of Winneshiek County, Iowa (Iowa Geological



A VIEW OF THE INSIDE OF THE MINE, SHOWING ICE-COVERED STEPS AT THE RIGHT.

Survey, Vol. 16, 1905, pp. 142 to 146), describes this phenomenon and quotes at length from Mr. Kovarik's article, with approval. See also "Glaciers and Freezing Caverns," by Edwin Swift Balch, Philadelphia, 1900, pp. 88, 89, 177, 136 to 161; also "Ice Caves and Frozen Wells as Meteorological Phenomena," by H. H. Kimball, *Monthly Weather Review*, Vol. 29, pp. 366 and 509, 1901.

The writer looked through these references hastily and from Balch's "Glacieres or Freezing Caverns" the following is taken:

The natives and peasants in the neighborhood of Glacière caves generally believe that the ice of caves is formed in summer and melts in winter. I have met with this belief everywhere in Europe; in the Eifel, Jura, Swiss Alps,

Tyrolese Alps, and Carpathians: and also occasionally in the United States. Peasants and guides tell you with absolute confidence: "The hotter the summer the more ice there is." The strange thing is that any number of writers—sometimes scientific men—have accepted the ideas and statements of the peasants about the formation of ice in summer, and have tried to account for it.

The belief of the peasants is founded on the fact that they scarcely ever go to any cave except when some tourist takes them with him, and, therefore, they rarely see one in winter, and their faith is not based on observation. It is, however, founded on an appearance of truth: and that is on the fact that the temperature of glacière caves, like that of other caves or that of cellars, is colder in summer than the outside air, and warmer in winter than the outside

ANOTHER VIEW OF THE INSIDE.

air. Possessing neither reasoning powers nor thermometers, the peasants simply go a step further and say that glacière caves are cold in summer and hot in winter.

Professor Thury tells a story to the point. He visited the Grand Cave de Montarquis in midwinter. All the peasants told him there would be no use going, as there would be no ice in the cave. He tried to find even one peasant who had been to the cave in winter, but could not. He then visited it himself and found it full of hard ice.

While the writer does not claim, as these peasants, that the heat of summer is the direct and only cause of the formation of ice, he does hold that it is an indirect cause and that the ice to be seen in the Sweden Valley Ice Mine is formed after the temperature outside the mine is far above the freezing point, and it is when the temperature outside is the highest that the ice is formed the most rapidly. The cause of this will be explained shortly.

The general skepticism regarding the existence of this phenomenon has been illustrated many times of late and has furnished the people of Coudersport with an endless source of amusement.

In the early part of the summer of 1911 a certain man of Detroit, Michigan, came to visit relatives in Coudersport. He was, of course, taken to see the ice mine, which was in its prime at that season of the year. Upon his return to Detroit he wrote a short article for one of the Detroit papers in which he told of this wonder that he had seen near Coudersport and offered to bet any one and every one $100 or more that his fictitious-sounding story was true. A millionaire ice manufacturer took the bet and eight other business men of Detroit followed suit. Two newspaper men were selected as stake-holders to decide the

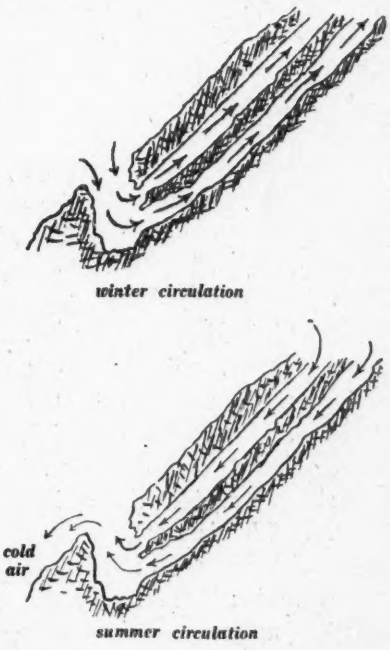
PETRIFIED WOOD TAKEN FROM THE SWEDEN VALLEY ICE MINE.

bets. They visited the mine and, of course, verified the newspaper story, much to the disgust of the nine losers.

It is claimed by a great many persons who hear of this phenomenon, never by those who actually see it, in the summer time, that the ice is not formed during the summer, but is only an accumulation from the preceding winter. It was to prove the falsity of this claim that the writer visited the mine many times during the winter and spring of 1912. The existing conditions were found to be as follows:

The pit or shaft is about eight feet in diameter by twelve feet deep and, as shown in the sketches, is located at the base of a steep hill. In the winter time the pit is comparatively dry and free from ice. The temperature inside is the same as that prevailing outside. In the

spring of the year, as the snow on the hillside begins to melt and the frost comes out of the ground, water naturally begins to trickle down the sides of the shaft, where, strange as it may seem, it is frozen in the form of small icicles. This freezing process continues, until by July the sides of the pit are completely covered with a coating of ice a foot or more in thickness. In the early fall the process stops and the formation of ice gradually melts. The sides of the shaft are of loose shale, in which there are numerous crevices extending back and up into the hill, the rock strata being rather sharply inclined. A draught of cold air, which at some places is strong enough to extinguish the flame of a small taper, issues from these fissures in the summer time. This draught is variable, being stronger on hot than on cool days. A heavy mist may also be seen rising out of the pit and floating off down the hill close to the ground. The temperature of the pit during the past summer varied between 25 and 32 degrees Fahrenheit.



The explanation of this phenomenon appears to lie in the cold currents of air issuing from the crevices of the rocks along the sides of the shaft. The air must gain access to these fissures at some other point, which must be at a higher altitude than that of the pit, as will be seen from the following discussion.

This being true, it is evident that in the winter time the column of air directly over the pit is cooler and consequently heavier than that in the rock passages. Therefore, it forces its way down into the pit and up through the rock strata, chilling the rocks to a great depth and storing up a vast quantity of "cold." We see, then, that the amount of "cold" which is stored up, or the depth to which the rocks are chilled at the beginning of warm weather in the spring, depends upon the length and severity of the winter.

As the warm weather comes on the column of air over the pit becomes heated and is displaced by the cold, heavy air flowing down out of the passages. This cold current of air freezes any surface water which flows over the edges of the pit and maintains a freezing temperature as long as the supply of "cold" in the hill lasts, after which the circulation of air ceases and the ice formation melts.

The place at which air gains access to these passages need not be a single opening, but consists, in all probability, of numerous small apertures, covered possibly by a thin coating of moss, loose shale or other porous substance.

In the summer time the warm outside air entering these apertures comes in contact with the rocks which have been chilled by the reverse currents of the preceding winter and in doing so gives up its heat to them, becoming specifically heavier. It then forces its way on down, displacing the warmer and lighter column of air above the pit.

It is evident that the rapidity with which this circulation takes place depends upon the difference in temperature of the two air columns. That is, the cold outward current is much more noticeable on hot days than on cool days in summer, and in winter the strongest inward current is noticed on the coldest days.

This fact accounts for the common belief that the freezing takes place more rapidly and that the mine is colder on hot than on cool days.

The temperature of the mine, or, in other words, of the air as it issues from the crevices, remains practically constant throughout the summer, which is proved by thermometer readings. However, the difference between this constant temperature and the temperature prevailing outside the mine is obviously greatest on the hottest days and therefore, as one enters the mine, the contrast is more noticeable. This causes one to believe that the mine is colder when it really is not. It is true, however, that the ice is formed most rapidly during the hottest weather. This is not because the temperature of the mine is lower, as is generally supposed, but is due to the fact that the circulation of air is more rapid; that is, a greater quantity of cold air issues from the numerous apertures and consequently a greater amount of "cold" is available for the formation of ice.

As soon as the supply of "cold" in the rocks is exhausted the internal and external air columns become gradually equal in temperature and weight, the circulation ceases and the ice begins to melt. This generally occurs about September of each year.

If this is the true explanation of this phenomenon, we may say, with truth, that in this particular instance it is the heat of summer which causes the ice to form, but, at the same time, we can not disregard the fact that it is the severity of the preceding winter and the natural arrangement of the rock strata which make it possible for the heat of summer to produce this peculiar phenomenon.

WHAT BECOMES OF THE LIGHT OF THE STARS?

By FRANK W. VERY
WESTWOOD OBSERVATORY

ONE of the most astounding things in nature is the enormous energy which the sun is continually dispensing as radiation to surrounding space. The earth, as viewed from the sun, is a mere point in space, and receives no more than 1/2,200,000,000 of the radiant energy which the sun is outpouring so lavishly. Yet out of this small fraction of the total radiation, practically all the terrestrial activities of wind and wave, tropical hurricanes and avalanches of ice on alpine slopes and the no less potent but milder forces which clothe the earth with verdure, originate.

If we include all the planets in the solar system, and assess the outgoing solar rays at the maximum tariff imposed by the obstructions in their path, it still remains true that only 1/100,000,000 of their power is directly utilized in maintaining the thermal equilibrium and life of the attendant orbs, dependent from day to day for these gifts upon the dispenser of all of this bounty.

The solar outpouring for even a single day is inconceivably great, yet the same flux of energy has been going on ceaselessly and with very little change in its absolute intensity for at least a hundred million years, as the records of geologic time attest. If only one part of solar radiant energy in one hundred million is directly utilized, what becomes of the other ninety-nine million, nine hundred and ninety-nine thousand, nine hundred and ninety-nine? Remember, also, that our sun is but one among hundreds of millions of stars made known to us by our photographic telescopes, all outpouring similar torrents of energy, and the question comes home to us accentuated with many million-fold intensity.

Professor Comstock[1] has shown that the theoretical and observed distributions of luminosity among the brighter stars may be reconciled, if we suppose either that the intrinsically brightest stars have a "distinct tendency to cluster about the sun," or else that "there is a sensible absorption of light in its transmission through space, of such average amount that a star having a parallax of a tenth of a second appears one magnitude fainter than it would appear in the absence of absorption." Other modes of attacking the problem must be invoked in order to decide between these alternatives.

[1] George C. Comstock, "The Luminosity of the Brighter Lucid Stars," *Publications of the Astronomical and Astrophysical Society of America*, Vol. 1, p. 307.

The much more searching analysis of Professor J. C. Kapteyn[2] favors an actual absorption of light from the more distant stars, but a very much smaller one than that demanded by Comstock's result. Kapteyn's method, however, when applied to bodies more remote than the nearer stars, gives about the same amount of absorption for the easily resolvable clusters, N.G.C. 7078 and 7089, and for the irresolvable and very much more distant *Andromeda* nebula, which indicates that his absorbent medium is a local adjunct to these stellar masses, and that it is perhaps a meteoritic envelope of somewhat greater volume than the stellar agglomeration, but not a universal medium filling all space. The circumferential absorption or scattering depletion of light by a limited envelope can not be taken as an indication of nebular distance, but will vary with the constitution of the enshrouding meteoritic swarm.

To make apparent any general absorption of radiation by the interstellar medium, it becomes necessary to investigate the properties of space far beyond the limits of the Galaxy and its outlying shells of sparsely distributed stars, and, crossing the immense voids of surrounding ether, to inquire whether they contain other galaxies of dimensions comparable with our own, and whether these afford any evidence of a gradual absorption of luminous energy by the intervening medium.

The first scientific enunciation of the doctrine that there are such external galaxies was given in 1734 by Emanuel Swedenborg in his *Principiorum Rerum Naturalium*,[3] and Herschel's nebular discoveries lent some support to the doctrine; but it was not until after 1864 that further evidence really bearing on the question came. Then, spectroscopic examination at the hands of Huggins and his successors divided the nebulæ into two great classes of the gaseous nebulæ with spectra of a few bright lines, and the white nebulæ with continuous spectra. This furnished the first real criterion for a fundamental distinction.

The gaseous nebulæ are so closely associated with the Milky Way that they obviously belong to our galactic system; and Ranyard's recognition of wide, dark lanes or spots, often branching or dendritic in form, blotting out extensive regions on Barnard's photographs of the Milky Way, showed that not all of the gaseous bodies in its neighborhood are luminous, but that some are to be compared to a dark smoke or mist, obscuring the glories of the brightness which lies back of the widely extended and absorbent cosmic cloud.[4] Among the con-

[2] *Contributions from the Mount Wilson Solar Observatory*, No. 42.

[3] "Emanuel Swedenborg-Opera quædam aut inedita aut obsoleta de rebus naturalibus nunc edita sub auspiciis Regiæ Academiæ Scientiarum Suecicæ. Holmiæ, 1908." II Cosmologica-Pars tertia, Paragraphus prima, N. 8 et 11, pp. 271–272.

[4] See A. Cowper Ranyard's completion of Proctor's "Old and New Astronomy," where the subject is discussed at some length, pp. 739–746.

spicuously vacant spaces in the Milky Way may be noted those running east from *Rho Ophiuchi,* others east of *Theta Ophiuchi,* and mingled star clouds and vacancies in *Sagittarius* near 18½ hours right ascension and 11° south declination.

Since there exist these enormously extended masses of gaseous or misty material, capable, whether themselves luminous or dark, of exerting a strong absorption upon the light of any bodies beyond them, and intimately associated with the Milky Way; and since, further, it is inevitable that the broad disk of the galactic accumulation must have gathered into its vicinity great swarms of meteoritic material,[5] acting after the manner of a general, widely distributed mist, forming an envelope analogous to an atmosphere having its greatest depth in the direction of the galactic plane; it follows that this extensive quasi-galactic atmosphere and its associated, but locally limited, gaseous bodies must especially absorb the light from those distant galaxies which lie in or near the plane of the Milky Way. This, it seems to me, is the probable explanation of the extraordinary increase in the numbers of the white nebulæ near the poles of the Galaxy, namely, that the galactic quasi-atmosphere being thinnest along a diameter at right angles to the plane of the swarm, the light of external galaxies is best able to penetrate through the obstructions if coming from this direction.

Kapteyn's recognition of absorption by an interstellar medium also supports the above explanation, since he finds that the absorption diminishes in extra-galactic latitudes.[6] Professor Comstock, it is true, reaches a different result, finding that stars of the 10.5 magnitude have larger proper motions as their galactic latitude increases, whence he concludes that "at right angles to the Galaxy the limits of the stellar system fall within the range of vision," which may be correct, but his explanation that this is so because "the transmission of light through and that this medium offers little obstruction in the direction of the galactic plane does not necessarily follow. The simple explanation that the Galaxy is a discoidal aggregation of stars with limits less remote than is sometimes assumed, permits the supposition that the 10.5-magnitude stars in the galactic plane comprise many relatively bright stars at a double distance and having a mean annual proper motion of 0″.01, whereas the extra-galactic stars are the extra-galactic spaces is impeded by some absorbing medium,"[7]

[5] The central regions of a galactic accumulation of stars may be expected to be relatively free from meteoritic material, for here we have a space swept clean by the stellar attraction which gathers in the material and places it where it can be readily absorbed. In the more distant intergalactic spaces, the meteoritic material is widely dispersed, but upon the borders of the galaxies there are accumulations of finely divided matter, not yet incorporated in the stars.

[6] *Contributions from the Mount Wilson Solar Observatory,* No. 42, pp. 23–24.

[7] *Publications of the Astronomical and Astrophysical Society of America,* Vol. 1, p. 282; see also *Astronomical Journal,* No. 558.

soon cut off by the external galactic limits and have a mean distance one half as great, represented by a double proper motion of 0″.02. This hypothesis fits the observations and reconciles the conflicting results of the two investigators.

Among the many spiral or discoidal nebulæ there are some which have the plane of the disk presented edgewise, and which are foreshortened into long and narrow shapes, sometimes with a central globular condensation. Several of these elongated objects are centrally divided by a dark band. I take it that these dark bands represent the quasi-atmospheric element in question. One of the best examples is the nebula Herschel II 240 *Pegasi,* which is a fusiform object (as seen in projection) with a strong central condensation, and fading gradually towards the extremities. The bright mass is almost exactly bisected by a longitudinal black band, sharply defined, and about one fourth of the width of the bright part near the ends. It appears to be an equatorial belt of absorbent material, outside of, or an extension of, the margin of a luminous lenticular mass. Other examples are: HV 19 *Andromedæ,* HV 8 *Leonis,* HV 41 *Canum Venaticorum,* HV 24 *Comæ Berenices* and HI 43 *Virginis.* It is very probable that our own Galaxy is a similar disk-like aggregation of stars, involving spiral star-streams, and surrounded or interpenetrated by an absorbing medium which is most extensive in the plane of the disk.

In considering the absorption of light in space beycnd the farthest reaches of the Galaxy, the investigation is best limited to luminous bodies of the galactic order which are neither themselves involved within the coils of our own starry system, nor situated in an extension of its plane, that is, we must exclude those objects whose galactic latitude is small. The latter, by the hypothesis, will consist of only a few near and relatively brilliant objects whose light has sufficient intensity to penetrate the galactic absorbent medium; but lest the distinction should be considered too fine, or too hypothetical, it may be waived in the present test.

I find only one nebula among those pictured by Mr. Isaac Roberts which is in a conspicuously vacant region. Of this nebula, H IV 74 *Cephei* = G.C. 4634 = N.G.C. 7023, Roberts says: "The nebula appears in a region almost devoid of stars." It is situated near the border of a branch of the Milky Way. Sir William Herschel has recorded *his* impression that nebulæ are apt to be found in regions which are poor in stars. This may be so, but an impartial examination of the photographs seems to indicate that the supposed connection between nebulæ and stellar vacuities is mainly a myth. It will require more extensive material than we now have to decide the point. Where such connection does undoubtedly exist, two different causes may be assigned for it: (1) a gaseous nebula *between* the Milky Way and ourselves may have a wide border of non-luminous absorbent material

which blots out the light of the more distant stars; and (2) the misty matter associated with the more condensed star-groups may obscure the light of external galaxies which therefore are better seen through the thinner places in our own stellar mass. Either of these causes would account for the stellar voids which Sir William Herschel describes as even a warning of the proximity of nebulæ; but it will be seen that there is no foundation for the inference which Mr. Herbert Spencer has built upon the supposed fact, namely, that none of the nebulæ can be external galaxies, *because* "thousands of nebulæ . . . agree in their visible positions with the thin places in our own Galaxy," and that they are necessarily most intimately linked with its structure. The connection, if established, will in no wise invalidate the wider generalization that external galaxies must appear to be most numerous in those regions where the mists or gaseous masses attendant on our Galaxy thin out and permit the light from the outside to penetrate the starry walls.

Mr. Roberts bears this testimony to the fact that the larger part of the nebulæ are situated beyond the confines of the Galaxy: There are "to be seen," he says, "stars apparently in a complete state of development, scattered over the surfaces of the most prominent of the nebulæ, but it will be observed that they do not conform with the trends of the spirals nor with the curves of the nebulous stars [or stellar condensations[8]?] involved in them. This fact I apprehend to be strong evidence that they are independent of the nebulæ—that they are not in any way involved in the nebulosity, but are seen by us either in front, or else in space beyond the nebulæ. If they were beyond them, their light would have to penetrate through the nebulosity, and we should therefore expect it to be duller in character and the margins of the stars to be surrounded by more or less dense nebulous rings; but these effects are not traceable in the photo-images, and we are consequently led to adopt the alternative inference that they are between us and the nebulæ. If they were involved in the nebulosity, they would conform with the trends of the convolutions and appear like nebulous stars."[9]

The dark lanes in the Milky Way are sometimes called "rifts," a term which implies that the stars are distributed in a relatively thin sheet which can be rent asunder. Moreover, the word is not used in a merely metaphorical or descriptive sense, but in its full significance, as in the following quotation from "Worlds in the Making" by Svante Arrhenius (p. 173): "The presumption that these rifts represent the tracks of large celestial bodies which have cut their way through widely expanded nebular masses has been entertained for a long time." And

[8] Of the larger spiral nebulæ, Professor G. W. Ritchey says (*Astrophys. J.*, Vol. 32, p. 32, July, 1910): "All of these contain great numbers of soft star-like condensations which I shall call *nebulous stars.*" It appears not improbable that these represent irresolvable stellar groups.

[9] "Photographs of Stars, Star Clusters and Nebulæ," Vol. 2, pp. 23–24.

the same author explains the dark ring around the nebula near *Rho Ophiuchi* on the supposition that "the smaller and more slowly moving immigrants . . . are stopped by the particles of the nebulæ," and are detained by the gathering crowd. But even if it could be demonstrated that the stars are arranged in thin sheets, and that celestial bodies exist of sufficient size and momentum to plow their way through great aggregations of stars, demolishing everything in their track, it would still be exceedingly improbable that only one layer of stars should exist in a given direction, or that several rifts should coincide. On the other hand, the presence of widely extended masses of dark absorbent matter in the shape of branching streams, sheets or rings, situated *between* us and the depths of star-strewn space, is not unlikely.

The gaseous nebulæ which form a part of the galactic structure are often very extensive, and are of a great variety of shapes, being frequently strangely irregular; but the more numerous white nebulæ are formed more nearly after a common pattern, although still with infinite variation as to details.[10] In general, what is common to nearly all of the white nebulæ is a tendency to form a two-branched spiral, the branches issuing from opposite sides of a central condensation, and coiling either within the boundaries of a plane circular disk, or forming a helix around a cylindrical directrix. The former figure is the more characteristic, and is well exhibited in the *Great Nebula in Andromeda.*

Another very remarkable and at present unique type is the transient nebulosity which appeared around *Nova Persei,* issuing from the star as a center, and expanding into the commencement of a vortical ring. It was an electric phenomenon, an exhibition of canal rays, or positive ions, on a grand scale. Facts from the history of these two bodies will be found useful in preparing one of the necessary means for our quest.

It is obvious that we require for this investigation of external galaxies some scale of distances, and equally obvious that at present such a scale can be only approximate. Indeed, it is probably this uncertainty as to the scale on which the universe is constructed which deters astronomers from attempting to discriminate between different galactic orders. I propose to see if this uncertainty can be, in part, removed.

I propose to take the distance of the *Andromeda* nebula as our celestial "yardstick," which may be called one *andromede,* and assuming that when we consider a large number of nebulæ, the average size does not vary with the distance, and that consequently the average distances may be taken inversely proportional to the angular diameters of the objects, I shall classify the nebulæ according to apparent size and brightness. It is essential that the subdivision shall not be too minute.

[10] The class of white nebulæ exhibits various stages of development, and includes objects of mixed type. See E. A. Fath, "The Spectra of Spiral Nebulæ and Globular Star Clusters," *Astrophysical Journal,* Vol. 33, p. 58, January, 1911.

There is in nature a tendency to wide variation, coupled with a coordinate tendency to uniformity in averages, when the number of classes is limited. Thus the land mammals range in size from elephants, say 15 feet long, to mice and shrews of a few inches. If we divide the earth into a good many faunal regions, the average sizes of the mammals in the different provinces may vary considerably; but if we divide the earth into only two halves, the averages will be almost identical.

For the present research, I take Sir John Herschel's "General Catalogue of Nebulæ and Clusters of Stars," which, coming from a single hand, and that the hand of a master, may be considered fairly homogeneous; and excluding the clusters which are known to be associated with the Milky Way, and are therefore comparatively near, I divide the remaining objects into two classes: (1) large nebulæ, or those having a diameter greater than 2′; and (2) small nebulæ, or those which are less than 2′ across; and I shall assume that the small nebulæ are on the average farther away than the large nebulæ in the ratio, $x:1$, leaving the value of the ratio to be determined by considerations to be drawn from the result, and which will appear in the sequel.

A point-source of light diminishes in brightness as the square of its distance increases; but light from a large number of points so close together that they can not be discriminated must be treated as a luminous surface; and since the angular area of a surface also diminishes proportionally to the inverse square of the distance, the intrinsic brightness, or the brightness of the unit of angular area, does not change with the varying distances of the nebulæ. We must therefore inquire: Is the intrinsic brightness of a small, and therefore presumably distant, white nebula equal to, or less than that of a large one? If the average brightness of the unit of angular area is less for the smaller white nebulæ the presumption is that the light of the smaller and more distant objects has been absorbed in passing through space. To apply this test, I further subdivide each class into three groups—(*vf*) very faint, (*f*) faint and (*b*) bright, or, if desired, the last two may be combined into a single group.

Dividing the nebulæ in Herschel's catalogue into groups of four hundred each, and taking the ratios of the small to the large nebulæ in each of the thirteen groups, I find that without exception the *faint* and small nebulæ are more numerous than the *bright* and small in a relatively very much larger ratio than occurs in the corresponding divisions of the large nebulæ. With only three exceptions the same relation is obtained by comparing the *very* faint and the *faint* nebulæ. Treating the groups separately, and taking the mean of the ratios, I find

Small divided by large: *vf*, 8.38; *f*, 6.83; *b*, 1.48.

The sums for the entire catalogue are

Small: $vf = 1765$, $f = 897$, $b = 241$.
Large: $vf = 235$, $f = 172$, $b = 204$.

The division into separate groups with the result that the same general law is given by every one of the groups is of course the more severe test; but taking the ratios for the sums as answering our present purpose, we have for the ratio of

$$\frac{\text{Small nebulæ}}{\text{Large nebulæ}}: \quad vf,\ 7.51; \quad f,\ 5.22; \quad b,\ 1.18;$$

or approximately $vf : f : b = 6 : 4 : 1$; that is, the very faint nebulæ are in excess over the bright ones among the small nebulæ in the ratio 6 : 1, but are of nearly equal frequency among the large nebulæ. In other words, the large nebulæ are *intrinsically* much brighter than the small ones.

I next performed the same operation with the 744 objects in a "Catalogue of New Nebulæ Discovered on the Negatives" taken with the Crossley reflector at the Lick Observatory, dividing them into two groups: (1) very small, or not over one half minute in diameter, and (2) those which are above this size and which may be called "large." These groups were divided into two classes: (*a*) very faint, including those which are described as "very faint" and "*very* very faint," and (*b*) pretty bright, or those given in the catalogue as "faint" to "bright." The result of this examination is that *three fourths of the large nebulæ are pretty bright, and one fourth very faint;* while the very small nebulæ have *just the opposite distribution of brightness, three fourths of them being very faint, and only one fourth pretty bright.*

In comparing the two catalogues, it must be recognized that the photographic method is far more delicate. Most of the objects in the photographic catalogue could not be detected by visual examination. The photograph also includes faint margins and therefore increases the apparent size of such nebulæ as are visually perceptible. Consequently, Herschel's small nebulæ are about equivalent to the "large" nebulæ of the photographic catalogue, and we should expect that the photograph would include a much wider range of brightness—all of which is confirmed by a discussion of the observations.

Let us suppose that the average distances of the several classes of nebulæ are given in andromedes, and denoted by the letter a, and that the coefficient of transmission of light through space is t^a; also that the mean distances are inversely proportional to certain assumed apparent diameters which are fairly typical. Each class of nebulæ includes objects having a considerable range of actual diameter, that is, the variation of distance is not as great as that of the apparent diameter. Instead of taking a mean value of $\frac{1}{4}'$ to represent the diameter of that class which includes nebulæ less than $\frac{1}{2}'$ in diameter, I take

the upper limit of $\frac{1}{2}'$ as representing the class of very small nebulæ. For the intermediate class which includes those objects called "small" by Herschel and "large" in the Lick catalogue, and which may be designated as *medium,* I take a diameter five times as great, or $2\frac{1}{2}'$; and for Herschel's "large" nebulæ, I take a diameter of 5′. The reason for these selections shall now be given.

I take for the diameter of the *Andromeda* nebula, 110′. This subtends the longer axis of the oval figure of the more condensed spiral arms. The fainter extensions are omitted because these are seldom included in the more distant nebulæ. Taking a suitable value for the coefficient of transmission, the curves giving the relation between brightness and distance become congruous for the two catalogues, if we take x, the unknown ratio of distance for large and small nebulæ, equal to 2 for Herschel's catalogue, and $x = 5$ for the Lick catalogue. This gives the following sequence:

Nebular Class	Diameter		Distance	Transmission
Andromeda	110′.0,	$a_1 =$ 1.0	andromede,	$t = 0.996$
Large nebulæ	5′.0,	$a_2 =$ 62.5	andromede,	$t^a = 0.778$
Medium nebulæ	2′.5,	$a_3 =$ 125.0	andromede,	$t^a = 0.606$
Very small nebulæ	0′.5,	$a_4 =$ 625.0	andromede,	$t^a = 0.082$

The statement which was made for the ratio of brightness among the groups in the Lick catalogue (vf and $f + b$ for large and small nebulæ) can be repeated in identical language for Herschel's catalogue by merely substituting the fraction $\frac{2}{3}$ instead of $\frac{3}{4}$; that is to say, Herschel's nebulæ are not only nearer than the Lick nebulæ, but are more nearly at a common distance; and the fraction expressing the ratio of brightness for the two groups of near and distant objects among the Herschelian nebulæ approaches nearer to the value of equality which it would have if all of the nebulæ were at the same distance, for then there would be equal absorption, and large and small objects should be equally grouped about a mean value.

Ratio of brightness for large and for small nebulæ	If equidistant,	1 : 1
	Herschel,	2 : 1
	Lick Obs'y,	3 : 1 [11]

The absorption exerted by the medium between us and the nebulæ is in the main a non-selective one. If it were not so, but resembled the ordinary selective absorption of the planetary atmospheres, the most distant nebulæ should be deep red instead of white. Some selective absorption may, however, be exercised by the misty quasi-atmospheric envelopes which we have reason to believe are associated with some or

[11] For the details of this investigation reference may be made to my paper, "Are the White Nebulæ Galaxies?" *Astronomische Nachrichten,* No. 4536, Bd. 189, 441–454, November, 1911.

all of the galaxies; but these effects will be local and independent of the distance separating us from the galaxy in question.

If the intergalactic absorption is non-selective, and therefore not to be attributed to diffraction from particles comparable in size with the wave-lengths of light, nor to selective scattering produced by gaseous molecules, to what shall we refer it? We believe, on what seems to be good scientific evidence, that meteoric stones and meteoritic dust particles are strewn through the celestial spaces. Can they produce the depletion of the nebular light?

In part, no doubt, the light is absorbed by meteoritic material; but there is a fatal objection to the supposition that all, or even a large part, of the absorption can be produced in this way. If there were enough meteoritic dust to reduce the light from the most distant nebulæ to a small fraction, only this fraction could escape absorption. The rest of the radiant energy from the stars would be absorbed and reradiated from particle to particle, but without being able to escape, and the entire mass of meteoritic material accumulated in the untold depths of space must eventually glow. Long before this, the skies would become a scorching envelope. The universe would be a prison house. There would be no escape from its brazen walls.

Is there any other solution of the problem? I think that there is; but first let us get an approximate conception of the dimensions of this universe of galaxies. By combining the rate at which the nebulosity around *Nova Persei* expanded, with established principles from known physical laws, and noting further that the nova, like all of its kin, was a galactic object—a member of the condensed swarm of stars which constitutes our Milky Way—also that it was on the more distant branch of that mighty ring, I have deduced a first approximation to the dimensions of the more condensed portion of our Galaxy.

Next, I have passed from the Milky Way to the Great Nebula in *Andromeda* by asking how much farther the nebula must be in order that a new star which appeared almost at its very center in August, 1885, should have been comparable in brightness with a nova of moderate size in our own Galaxy. The answer is that approximately 1 andromede = 1600 light-years, or 15,000,000,000,000,000 kilometers.[12] An entirely independent computation, on somewhat different lines, by Mr. J. Ellard Gore, leads to a result of the same order. Mr. Gore is not quite as explicit as I have been; but the general agreement between our results makes me feel confident that we are not far from the truth.

No other of the white nebulæ compares with the *Andromeda* nebula

[12] In *Knowledge* for September, 1912, I conclude that Lord Kelvin's estimate of the diameter of the Galaxy, which was five times as great as mine, is probably the better of the two, whence it follows that 1 andromede = 8,000 light-years. But we are concerned at present with rough estimates of an order of magnitude only, and may waive all minute details.

in size, and their average distance apart may perhaps be ten times as great. We will suppose that each galaxy is at the center of an otherwise unoccupied cube 10 andromedes on an edge. The radius of a sphere containing 450,000 such cubes is 760,000 light-years. Now Perrine estimates that there are at least 500,000 nebulæ in the heavens within reach by the Crossley reflector, and probably nine tenths of these are white nebulæ or galaxies. It is therefore safe to say that the light of the stars can travel for one million years before becoming so much reduced by intergalactic absorption as to be beyond the grasp of this powerful instrument.

The view which I now wish to present is that it is the ether itself which absorbs the radiation from the stars.

Considered merely as to its volume, the ether is so overwhelmingly immense that all other bodies shrink into nothingness in comparison. The radius of the sun is

$$r_{\odot} = 7 \times (10)^5 \text{ kilometers.}$$

Half the distance to the nearest star is

$$r_{*} = 2 \times (10)^{13} \text{ kilometers.}$$

An ethereal sphere which may be called the sun's own, being bounded by the similar spheres of neighboring stars, may be drawn with the latter radius. The radius of the sun bears to that of its interstellar sphere the ratio

$$r_{\odot} : r_{*} = 1 : 30{,}000{,}000,$$

and the volume of the associated ether exceeds that occupied by the solar substance in the ratio

$$(r\ )^3 : (r_{\odot})^3 = 2.7 \times (10)^{22} : 1.$$

Since there are vacant spaces between neighboring galaxies, something must be allowed for these. Let us suppose that the ethereal volume is four hundred times greater than that just given, or that its volume ratio is

$$\text{Ether volume} : \text{Matter volume} = (10)^{24} : 1.$$

This allows a considerable extension of thinly scattered stars around each galaxy, and places the galaxies at relatively smaller distances from each other than the stars, if distances are expressed in terms of diameters, an arrangement which is indicated by the evidence already presented.

The next step in the argument demands an estimate of the total light from all of the stars. Call this L. Newcomb gave us such a photometric measurement, and found

$$L = 600 \text{ stars of zero magnitude.}$$

The brightness of the sun is

$$L' = 3.3 \times (10)^{10} \text{ stars of zero magnitude.}$$

Hence $$L' = \frac{3.3 \times (10)^{10} \times L}{600} = 5.5 \times (10)^{7} \times L.$$

The average illumination in intergalactic space is very likely less than one one-hundred-millionth of that of sunlight; but a majority of the stars have less absorbent atmospheres than our sun, and as sunlight at the earth's distance must be increased in the ratio 1 : 46,000 to give the light emitted by the surface of the solar sphere, the average radiant energy at stellar surfaces may be assumed as $(10)^{12}$ times the average radiant energy in the star-lit ether.

If V and L are the volume and average illumination of the ether, V' = the total volume of stellar material, and L' = the total light from the combined surfaces of all of the stars, an instantaneous image of the relation between the two bodies—ether and matter—that is to say, a representation of the relation if there were an instantaneous emission of light with an infinite velocity, would give

$$VL : V'L' = (10)^{12} \times 1 : 1 \times (10)^{12},$$

or equality. But if the element of time enters, and also the actual velocity of light, the illumination at a given point in the ether will increase with the time. Let the year be the unit of time. After one billion years, supposing that the stellar radiation can have endured as long as this, instead of unity for the ratio $VL/V'L'$ as in the preceding equation, we shall have

$$VL = V'L' \times (10)^{12}.$$

Considering the limiting surface of the ether to be, not an imaginary circumscribing sphere, but the sum of the combined stellar surfaces across which the sum total of stellar radiant energy is being constantly transferred from matter to ether, the case stands about like this:

Volume	Radiation (Superficial)	Total Radiant Energy (Volumetric)
Stars = 1	Stars = $(10)^{12}$	Stars = $(10)^{12}$
Ether = $(10)^{12}$	Ether = $(10)^{12}$	Ether = $(10)^{24}$

The large amount of the total radiant energy of the free ether, compared with that of the stars may seem surprising, but it results from the fact that the average illumination of the ether is due to the accumulation of radiant energy from depths of space which are greater as the ether is more transparent. Unless the radiant energy were absorbed, it could not do otherwise than accumulate. The accumulation represents the combined radiation of an immense number of stars whose average distance is to be measured in millions of light-years—how many millions depends upon the time that the stellar radiation remains in the ether before it is all absorbed.

According to what precedes, the average ethereal energy can hardly be less than the radiant energy from the stars within a range of a million light-years, and may amount to many times this figure; and as

the absorption is a gradual one, the actual duration of luminous propagation may have to be reckoned in thousands of millions of years. Now the radiant energy of the ether represents its temporary mass. If we knew the relation between mass energy and radiant energy, we could give the ratio between the permanent energy of mass of the stars and the luminous energy of the ether. For example, if the mass energy of a star is on the average $(10)^{12}$ times its radiant energy, then the total energy of the universe is always equally divided between ether and matter, because the same radiation comes forth from unit volume of matter, and is distributed to $(10)^{12}$ units of ether. Or, if mass energy bears a larger ratio to radiant energy than this, energy may remain longer in its material than in its ethereal form, only a small fraction of the total energy residing in the ether.

To conjoin stellar centers and ethereal expanses, an intermediate order of existence is needed: An order which faces both ways, having relations with the ether and with the stars. Viewed from the side of ether, we begin to dimly apprehend an electric substance, not yet matter, although possessing many of its properties, seeming to be both a substance and a force, mobile, energetic, viscid enough to be localized and to take on intricate forms, a world-plasm, waiting to be incorporated.

Meteorites circulating around a galactic center remain for enormous periods in the neighborhood of their apogalacteum, and moving with extreme slowness, they have time to gather to themselves the scattered atoms of space, even though the attracting masses may average only a few grams. A meteoritic mass of 1 gram which, if quiescent, will attract to itself the particles within a radius of 1 meter in about 2 months, may be expected to leave a clean-swept track of considerable width through that part of its revolution which occurs in intergalactic space. Possibly the meteoritic chondri have thus grown by accretion in the depths of space, even if, as some suppose, their nuclei may have originated by condensation from masses of heated mineral vapor. Such a slow growth is not incompatible with various vicissitudes, and an eventual consolidation of many such masses into compound chondritic complexes, after the manner of the formation of large hail stones.

Particles which are thrown off from luminous stars, or from fine cosmic material near the stars, being driven away by the pressure of light, are not necessarily of dimensions much larger than molecular, and although the swiftness and small mass of such light-repelled particles must prevent them from acquiring additions by attracting the atoms near which they pass, some increase of size is to be anticipated by chance collisions with atoms, the particles being slowed down and reabsorbed by massive attracting bodies. But these are the last steps of an intergalactic process. We must go farther back to reach its inception.

If we attribute the absorption of light in space to the ether itself,

the radiant energy absorbed performs work upon the ether, presumably the generation of minute ethereal vortex-rings—the elementary particles from which electrons are derived, or possibly the positive and negative electrons themselves out of which the atoms are formed. From associations of electrons to atoms, from atoms to molecules, from molecules to the first tiny beginnings of a cosmical crystalline sublimate, there is a continual progression and increase of size. Finally, this widely dispersed material must be gathered from the immense voids of space into the germs of future worlds, and for this task the meteorites appear to be the appointed instruments.

A process which goes on forever in one direction is inconceivable. For every swing of the pendulum there must be a counter swing. If atoms have been built up by the action of light, they can be torn apart, and the energy of their formation will be once more set free. We may assume that a certain proportion of the atoms disintegrate, a very minute proportion ordinarily in planetary bodies, but a much larger one under solar conditions. The following facts suggest a relation: (1) The known radioactive elements disintegrate with the production of helium, and the evolution of enormous thermal energy. (2) The stars which are, at least externally, the hottest, since they have effective temperatures which have been rated in a few cases as high as 40,000° C., are surrounded by extensive atmospheres of helium. These relations favor the hypothesis that the helium stars contain an exceptional amount of peculiarly unstable elements, and owe their high temperature to the heat set free in the gradual elimination and destruction of these substances. The energy of formation of the atoms is being slowly dissipated as radiation from the stars, but is eventually reabsorbed by the ether, and is thus restored to the material phase of its existence by the formation of new atoms.

A plausible inference may be formed from the behavior of radium. In 1,000 years, 4 grams of radium will have been nearly one third transformed into other forms of matter of less intrinsic energy, the radium being reduced to about 2.8 grams. During this interval of time, the four grams of radium will have emitted, according to Rutherford's measurement of the annual production of heat from radium,

$$\frac{(4.0+2.8)}{2} \times 876{,}000 \times 1{,}000 = 3.0 \times (10)^9$$

gram-calories of heat. This is, of course, only a first approximation. The progression is not strictly linear. Since the gram of substance transformed has not, in this case, been annihilated as matter, but has simply been transmuted into other forms of matter, the $3 \times (10)^9$ gram-calories of thermal energy do not represent the total mass-energy of the gram of matter, but only that portion of the mass-energy which has been lost in this partial transformation. If we suppose that the total original energy is 1,000 times as much as that which has been lost

in 1,000 years by radio-active transformation, or enough to last at the same rate for 1,000,000 years, the thermal energy corresponding to the mass-energy of one gram is $3 \times (10)^{12}$, which is very nearly the same as the $5 \times (10)^{12}$ water-units, computed by De Volson Wood for the specific heat of the ether.[13] We seem, at any rate, to be approaching limiting values which are perhaps connected with the transition from ether to matter, or the reverse. If a volume of rotating ether, having a specific heat of $5 \times (10)^{12}$, can be condensed, or in any other way transformed into a volume of matter with specific heat unity, since specific heat is capacity for absorbing thermal energy, the tremendous shrinkage of this capacity during the formation of matter out of ether represents the absorption of so much energy, and the almost complete saturation of the original capacity. It follows that if the process is reversed, the thermal energy of atomic formation must be set free.

Since radium decays far more rapidly than most elements, the one million years suggested in the preceding illustration must be greatly extended in order to represent the average duration of matter. Similarly, the one million light-years deduced for the distance of the fainter nebulæ on the Lick Observatory plates is not a limiting distance beyond which light can not penetrate, but it is a distance at which light is reduced to perhaps eight per cent. of its original intensity, or a quantity of that order. It is evident from the phenomena connected with the decay of the radio-active elements, that different elements have different durations. The rarer elements are either those which require a very long time and a long process of successive ethereal modifications in their development, or else they are elements which are relatively unstable, and which decay more rapidly than the others.

Rutherford gives the radius of an electron as $1.4 \times (10)^{-13}$ cm., on the supposition that the electron is a sphere, in which case its surface will be $2.5 \times (10)^{-25}$ sq. cm., and its volume $1.1 \times (10)^{-38}$ cub. cm. The mass of an electron being, according to Sir J. J. Thomson, 1/1700 times that of a hydrogen atom, and the latter weighing $1.1 \times (10)^{-24}$ gram, the density of an electron works out

$$D = \left(\frac{1.1 \times 10^{-24}}{1.7 \times 10^{3}}\right) \div (1.1 \times 10^{-38}) = 5.9 \times 10^{10} \text{ (water} = 1).$$

This value is so extraordinary that obviously we are not dealing with any ordinary problem in material density. The only phenomenon which has any resemblance to it is the increment of mass which the electron acquires at velocities approaching that of light in Kaufmann's experiment. Add to this the fact that the velocity of light is a constant, and the conclusion apparently follows that if the velocity of wave-motion in the ether can be diminished to even the smallest extent below that of light, the medium ceases to be ether, and the motion ceases to be ethereal wave-motion, but is left behind as the beginning

[13] *Philosophical Magazine* for November, 1885, pp. 402–403.

of a materialized etheric energy. The enormous density found for the electron is an average density and must be still more exceeded if the mass-giving energy is not distributed uniformly within the volume. By all electric analogies it is natural to assume a superficial concentration of energy in the electron itself. The large apparent density of the electron is perhaps explicable on the assumption that the mass-giving substance is condensed in a very thin surface-layer where it revolves with a velocity smaller than that of light by only a very minute amount. The substance of such a shell should have an almost infinite density. The average density of the enclosed volume should still be very great. If, for example, the electron is a vortex-ring of ether of the same surface as the sphere, an almost infinitesimally thin shell of ether revolving ever so little slower than the velocity of light, is no longer free ether, but becomes matter of almost infinite density, the velocity-gradient falling off very rapidly in the interior of the vortex, and the internal density being negligible. Such a body should possess surface potential, polarity, strong elastic resistance, and other properties demanded of the electron.

If it be admitted that a definite volume of ether can receive a permanent limit, it seems necessary that some surface of discontinuity, as well as a stress, akin to fluid viscosity, exerted between the volume and its surface, should be set up. Calling E the ethereal viscosity, A the surface of discontinuity, and V a velocity, such as the mean velocity in the volume, or the limiting velocity at the surface, to be determined by the nature of the viscous mechanism which is at present unknown, the viscous stress (F), so far as it depends on dynamic considerations, is equal to a momentum transferred through a definite volume of fluid to a limiting surface at a given speed, and may be represented as in fluid viscosity by the equation

$$F = EAV,$$

but with this distinction: The ether has no mass except as it acquires mass by receiving a rotary motion. The dimensional equation for viscosity,

$$E = M/LT,$$

becomes

$$E = L^3 \times \frac{L^2}{T^2} \times \frac{1}{LT} = \frac{L^4}{T^3},$$

since the etheral mass is proportional to the energy (which varies as the square of the velocity) impressed upon a volume proportional to r^3, where r is the mean radius of the gyrating volume. In the case of a ring rotating in its own plane, or of a surface rotating around an axis which is a closed curve, r may be the radius of the ring or of the surface. Substituting the value of E in the expression for F, we have

$$F = \frac{L^4}{T^3} \times L^2 \times \frac{L}{T} = L^3 \times \frac{L^4}{T^4},$$

$$F \propto r^3 \times v^4.$$

The ethereal viscosity being excessively small, either very high velocities, or very long durations are required to produce appreciable ether drift. As Lagrange has demonstrated, there can be no surface of discontinuity in a perfect fluid, because such a surface implies a continuous generation of rotation in portions of a fluid of constant density. Conversely, if any discontinuity can be imposed upon the ether, it must be a viscous fluid. Any structures formed from a viscous fluid must eventually decay. The duration of the material phase may be enormous, but its ultimate transition is inevitable. The point I wish to make is that there is evidence of an absorption of light by the ether, and that there is also evidence of atomic disintegration. The two processes interlock into necessary and concomitant parts of a consistent whole. What I have tried to demonstrate is the existence of a phenomenon and its approximate law, without attempting a refinement which would be unwarranted at the present stage of the investigation.

Conclusion

In brief, we may conclude that space contains myriads of galaxies which would make the midnight sky one blaze of light, were it not for the absorption of light by the ether of space. This absorption can not be a selective scattering by gaseous molecules, because this would deplete the radiation of short wave-length unduly, and would redden the light of the more distant nebulæ, whereas no such change of color with distance is found. Neither can the absorption be due to the general absorption of radiation of every wave-length by coarser meteoritic dust, since the meteoritic material would in time become heated to incandescence, as Arrhenius has noted, and in this case also the entire heavens must glow. There remains, then, the supposition that the ether itself absorbs the radiation from the stars, and that in this fixation of energy, matter originates.[14]

There is, I apprehend, a close analogy between the sequences of cosmogony and of geogeny. Upon the earth there are wide expanses of oceanic depths which have apparently remained such from the beginning of denudation. That remarkable property of saline solutions whereby suspended solid particles are quickly precipitated, causes the marginal deposition of those sediments brought to the sea by the rivers. The oceanic depths are the counterparts of the intergalactic spaces. In both, change progresses very slowly.

But around the borders of the continents, sediments accumulate in geosynclines which are self perpetuating. The increasing weight of the deposit deepens the depression, until after the accumulation has

[14] As suggested in my paper, "A Cosmic Cycle," *Am. Jour. Sci.*, Ser. 4, Vol. 13, p. 189, March, 1902.

reached a depth of 10 to 15 kilometers a reaction sets in. The deeply buried beds of water-bearing detrital formations soften, very likely under the influence of heat generated by the concentrated radioactive minerals, as Professor Joly supposes.[15] Long eras of crumpling, elevation and mountain-formation follow, to be in turn succeeded by other ages of denudation. "The energy which determines the place of yielding and upheaval, and ordains that the mountains shall stand around the continental border," passes through a rhythmic interchange or cycle. The cosmogonical process which I have described embodies an analogous cycle, embracing the formation of matter from the ether, and most abundantly in the vicinity of stellar aggregates, by the fixation of the radiant energy, outpouring from the disintegrating stellar substance. Then follow, in turn, the concentration of the material on the borders of the earlier galaxies and the birth of new heavens. In proof of this association of old and new along a border region, the similar distribution of the fourth-type and helium stars, which probably represent the extremes of a thermal series, may be cited.

The conception of a universal ether is to many so vague that the distinction between ether and a purely spiritual atmosphere seems slight; yet the difference is fundamental. The mind of man is not conditioned by space. Thought can not be measured by the yardstick. Ether, on the contrary, occupies space. The dimensions of its waves have been made the fundamental standards of our units of length. Nevertheless, we still grope and guess as to the real structure and nature of the ether. Some of its properties seem to verge on the metaphysical. Back of it, we have glimpses of a source of energy which is inexhaustible, as if it were most intimately linked with the Infinite Source of all existence. Matter which used to be looked upon as dead, and as incapable of exhibiting energy except as this was thrust upon it from without by physical forces, begins to look almost alive. "It moves," said Galileo, of the solid earth; and to-day the delighted physicist, armed with the spectroscope and spinthariscope, Crookes's tube and the electrometer, finds, in the Zeeman effect or the radium emanation, evidence that the atom is an orderly maze of bewildering motion. Its inertia is a gyroscopic inertia. Absolute rest would be nonentity. Everywhere the universe speaks of never-ending life and motion. Creation is not the bringing forth of an infinite number of dead structureless particles, sent out as a set of miserable little waifs at some indefinitely remote epoch and left to clash without guidance, without purpose. Creation is perpetual. The interiors of matter are seen to be more and more wonderful, more and more intensely active, as we approach the sacred portals where divine influx from the Soul of the Universe quickens into the energy which is matter.

[15] J. Joly, "Radioactivity and Geology. An Account of the Influence of Radioactive Energy on Terrestrial History."

THE PROGRESS OF SCIENCE

THE ACADEMIC SITUATION.

SCARCELY a month passes without the occurrence of one or more events disquieting to those who would make our universities the homes of scientific research, creative scholarship and social progress. Such circumstances do not usually become known, for it is to the private advantage of those concerned that they be hushed. Strange as it may seem at first sight, the state universities are on the whole making progress in the direction of greater academic freedom and dignity, while the private corporations tend to exhibit the reactionary tendencies of their boards and administrative officers. If, however, the people learn the importance to the nation of maintaining their universities on a high plane, all is well. It is easy to tax corporations which become antisocial into innocuousness. Indeed each university will find its own level by its own weight. Harvard and Columbia are still our richest institutions and probably still maintain their leadership in advanced work and public service; but they are losing ground relatively to the state universities and perhaps even in comparison with their own positions ten years ago. It would surprise most people to see the list of those who have recently declined to consider chairs at these two universities.

It is the high traditions of Harvard which give significance to the curious circular recently sent from the controller's office to those whom one university president habitually calls "the instructional force." The circular is accompanied by four large pages of instructions and a schedule containing some 180 blank spaces to be filled and is couched in jargon about "prorating salaries to the various classified functions," and the like. The professors and instructors are informed that

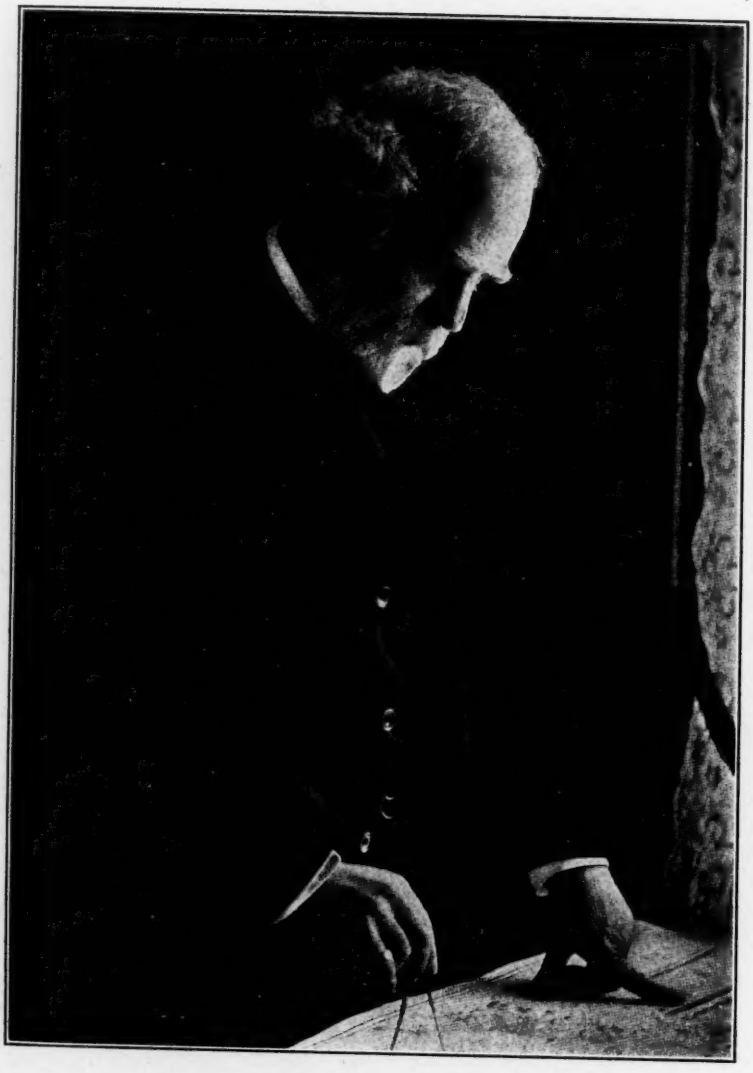
$(2a \times 3a) + (2b \times 3b) + (2c \times 3c) + (2d \times 3d) + (2e \times 3e) =$ total hours of regular exercises per course.

They are told that

> Preparation for lectures should include only that time which was taken during the half-year for lectures delivered in this period. It should not include time spent in the general collection of materials.

Surely the only correct answer to the question how many hours a day a professor spends on his work and in preparation is twenty-four. This circular was naturally resented by members of the faculty and was partially, but somewhat grudgingly, withdrawn, the president stating that it was "issued under a misunderstanding," presumably a misunderstanding of the sentiments of the faculty.

This Harvard incident is serio-comic. At Wesleyan there has occurred within the same past month a wholly serious breach of academic decency. The professor of economics and social science, who has served the university and the public with distinction for twenty years, made some remarks in regard to the observance of the sabbath, which found their way into the newspapers. The president wrote inquiring whether he was correctly reported, and on being told what he had said, asked for his resignation. This was promptly sent, and the president relieved him from his duties at once. The five letters passed in the same day, and the president must have acted without adequate consultation or consideration. It is as extraordinary as it is ominous that in our present academic system the liberty of speech of a professor and the fate of his wife and children should be dependent on the will of an official. In this case the professor was speaking within his own professional field, and not even to students of the university



SIR JOHN MURRAY.

or in its city. He surely would fare ill at Wesleyan University who said "The sabbath was made for man, and not man for the sabbath" and "Beware of the scribes which . . . for a pretence make long prayers; these shall receive greater damnation." The trustees of Wesleyan University still have the opportunity to decline to accept the resignation of the professor of economics and social science. The other honorable alternative is to change the name of the institution to the "Middletown Methodist College."

THE DEPTHS OF THE OCEAN

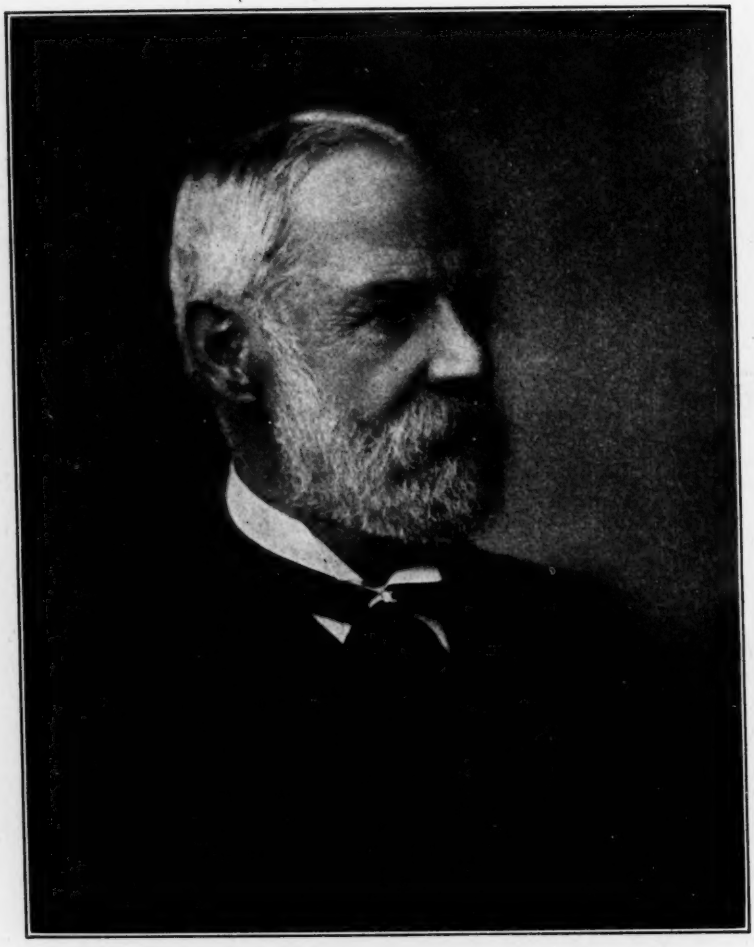
OCEANOGRAPHY as a science may be dated from the voyage of the *Challenger* round the world from 1872 to 1876 under the scientific direction of Sir Wyville Thomson and the naval command of Sir George Nares. Sir John Murray was one of the naturalists of the expedition and later became editor of the great series of reports. In addition he has published many important papers on oceanography and marine biology and has conducted surveys in marine and inland Scottish waters. Probably Sir John Murray and Alexander Agassiz are the two men who have accomplished the most for marine biology, and it is a cause for pride that both were born on this side of the Atlantic. We may also view with gratification the earlier work of Bache of our coast survey and of Maury of our navy, who in the forties and fifties laid the foundation on which the science of oceanography has been erected. When Sir John Murray visited the United States last year and made a series of extremely interesting addresses in various places, he established a fund in honor of Alexander Agassiz, under the National Academy of Sciences, for a medal to be conferred for distinction in oceanographic research. It should give us pause to reflect that there is none so well deserving this medal as were Dana, Bache, Maury and Agassiz.

In 1909 Sir John Murray—who like Agassiz acquired wealth by an incidental use of his scientific observations—offered to defray the expenses of a cruise of the *Michael Sars* in the North Atlantic, if the Norwegian government would lend the ship and its scientific staff. The expedition was undertaken with the cooperation of Dr. Johan Hjort, director of Norwegian fisheries. The *Michael Sars*, named in honor of the naturalist who sixty years ago made dredgings off the coast of Norway, was admirably equipped for deep-sea explorations. Starting from the east of Ireland it worked down to the Canaries and by way of the Azores to New Trinidad and back to Ireland and Bergen. About 120 observing stations were established and much valuable information was obtained, while the biological material has been distributed to specialists in different parts of the world.

A general account of the researches undertaken by the *Michael Sars* and of the modern science of oceanography has now been prepared by Sir John Murray and Dr. Hjort and has been published by The Macmillan Company. The book contains some 600 illustrations, the portrait of Sir John Murray being here reproduced, and forms an accurate and readable account of what is known in regard to the depths of the oceans of the earth.

GEORGE HOWARD DARWIN

SIR GEORGE DARWIN, of whose death we learned not long ago, was, perhaps as much as any of our times, one of the most noteworthy examples of the best scientific lives of our generation. Sprung from a family with notable scientific traditions for several generations, and gifted with talents in no way inferior to the best of those amongst whom he worked, he employed all the resources at his command for the promotion of the highest interests both of his own subject and of the



SIR GEORGE DARWIN.

scientific world at large. Never very robust in health, he accomplished several long and laborious tasks and yet rarely failed to place his time and energy at the disposal of those who made demands on them. He was brought up in a school of mathematics which put ingenuity and brevity at a high premium, yet when faced with a difficult problem he usually chose the direct route towards the solution, often at the cost of long and laborious calculations. Even when deeply engrossed in the work he was doing, he would lay it aside at a moment's notice to listen to and discuss the problems of his friends or pupils. And while acting as an inspiration to many of his contemporaries, he never failed to impress them with his modesty even when expressing his own opinion in his direct but kindly manner.

For many years Sir George Darwin has been recognized as the leader—a title he would have immediately disclaimed—in a subject which is perhaps the most fascinating and the most dangerous of all those which may occupy the thoughts of a scholar. Cosmogony is replete with unsolved problems and hypotheses may be multiplied almost indefinitely. Almost any new discovery or advance in our knowledge of the physical world may have a bearing on it. Sir George Darwin, whose best known work lies in this field, never allowed himself to be led much beyond what he was able to establish by exact methods. If he gave a theory of the past history of the earth and its satellite, he did not allow the reader to imagine that he had solved the problem, but simply considered his work as sufficient to make probable a possible hypothesis.

While his earlier interests were in the direction of pure science, his association with Lord Kelvin led him to the consideration of a practical problem. Tidal prediction is always important for a country with the mercantile interests of Great Britain. Sir George Darwin had immense power in dealing with long and intricate calculations, and his ability was nowhere better employed when he drew order out of chaos in furnishing methods which could be used by a seaman to obtain the tides of his port of call or by a government in the formation of tide tables for its coasts. This same facility and his gathered experience led to his advice being continuously sought in the discussion of meteorological records. In geodetic problems he was one of the chief advisers of the government and was its representative in the international congresses which have been held in Europe during the last fifteen years. In all such matters the English government asks for and acts upon the opinions of its representative scientific men, and Sir George Darwin took his full share in these responsibilities.

His most notable public function was his presidency of the British Association during the memorable tour in South Africa some seven years ago. The sounds of the warfare in that country had only just ceased and great tact was needed to avoid any unpleasant feelings either amongst the native or white races. It is not too much to say that the association could hardly have made a better choice for its presiding officer. In some forty speeches all over the colonies, while avoiding platitudes, he hit the right note, not stirring up excitement and not sending his hearers away without some thought which characterized the occasion. The same touch was visible in his final public appearance as president of the Mathematical Congress held in Cambridge last August. None of those who heard his tribute to Henri Poincaré on that occasion realized that he himself would so soon also depart.

His numerous friends not only in England and Europe, but also in this country, will regret the passing, not alone of the student, but of the wise and kindly man whose humanity was never lost in his scholarship.

SCIENTIFIC ITEMS

WE record with regret the deaths of Mr. Francis Blake, inventor of the telephone transmitter and other electrical apparatus; of Dr. Thomas Volney Munson, who while engaged as a nurseryman at Dennison, Texas, made valuable experiments on the breeding of fruits, especially in viticulture; of Professor George Augustus Koenig, professor of chemistry at the Michigan College of Mines; of M. Louis Paul Cailletet, the distinguished French chemist, known especially for his work on liquefaction of gases; of M. Léon Teisserenc de Bort, the French meteorologist, known for his work with captive balloons; of Dr. Otto Schoetensack, professor of anthropology at Heidelberg, and of Dr. Yujiro Motora, professor of psychology at Tokyo.

THE Elisha Kent Kane gold medal of the Geographical Society of Philadelphia was presented to Professor William Morris Davis, of Harvard University, on January 28, and the Culver medal of the Geographic Society of Chicago, on February 19.—Professor George Herbert Palmer, Alvord professor of natural religion, moral philosophy and civil polity, and Professor Francis Peabody, Plummer professor of Christian morals, have given their final lectures at Harvard University. Professor Palmer has served the university for forty-three years and Professor Peabody for thirty-eight years. —Professor J. Hadamard, professor of analytical and celestial mechanics in the Collège de France, has been elected a member of the Paris Académie des Sciences in the section of geometry, in succession to the late Professor Henri Poincaré.